SCIENCE

A WEEKLY JOURNAL DEVOTED TO THE ADVANCEMENT OF SCIENCE, PUBLISHING THE OFFICIAL NOTICES AND PROCEEDINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

FRIDAY, JANUARY 2, 1903.

CONTENTS:

MSS. intended for publication and books, etc., intended for review should be sent to the responsible editor, Professor J. McKeen Cattell, Garrison-on-Hudson, N. Y.

THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

*THE SCIENCE OF ASTRONOMY.**

I TAKE for the subject of my address the science of astronomy, and propose to give a brief historical sketch of it, to consider its future development, and to speak of the influence of the sciences on civilization.

The science of astronomy is so closely connected with the affairs of life, and is brought into use so continuously and in such a systematic manner, that most people never think of the long labor that has been necessary to bring this science to its present condition. In the early times it was useful to the legislator and the priest, for keeping records, the times of public ceremonies and of religious festivals. It slowly grew into the form of a science, and became able to make predictions with some certainty. This was many centuries ago. Hipparchus, who lived 150 B.C., knew the periods of the six ancient planets with considerable accuracy. His periods are:

	Period.	Error × 100 / Period.
Mercury	$87^d.9698$	$+0^d.0007$
Venus	224.7028	+ 0.0009
Earth	365.2599	+ 0.0010
Mars	686.9785	— 0.0002
Jupiter	4332.3192	— 0.0061
Saturn	10758.3222	— 0.0083

* Address of the President of the American Association for the Advancement of Science, Washington meeting, December 29, 1902.

These results indicate that more than two thousand years ago there existed recorded observations of astronomy. Hipparchus appears to have been one of those clear-headed men who deduce results from observations with good judgment. There was a time when those ancient Greek astronomers had conceived the heliocentric motions of the planets, but this true theory was set aside by the ingenious Ptolemy, who assumed the earth as the center of motion, and explained the apparent motions of the planets by epicycles so well that his theory became the one adopted in the schools of Europe during fourteen centuries. The Ptolemaic theory flattered the egotism of men by making the earth the center of motion, and it corresponded well with old legends and myths, so that it became inwoven with the literature, art and religion of those times. Dante's construction of Hell, Purgatory and Paradise is derived from the Ptolemaic theory of the universe. His ponderous arrangement of ten divisions of Paradise, with ten Purgatories and ten Hells, is said by some critics to furnish convenient places for Dante to put away his friends and his enemies, but it is all derived from the prevailing astronomy. Similar notions will be found in Milton, but modified by the ideas of Copernicus, which Milton had learned in Italy. The Copernican theory won its way slowly, but surely, because it is the system of nature, and all discoveries in theory and practical astronomy helped to show its truth. Kepler's discoveries in astronomy, Galileo's discovery of the laws of motion and Newton's discovery of the law of gravitation, put the Copernican theory on a solid foundation. Yet it was many years before the new theories were fully accepted. Dr. Johnson thought persecution a good thing, since it weeds out false men and false theories. The Copernican and Newtonian theories have stood the test of observation and criticism, and they now form the adopted system of astronomy.

The laws of motion, together with the law of gravitation, enable the astronomer to form the equations of motion for the bodies of our solar system; it remains to solve these equations, to correct the orbits, and to form tables of the Sun, Moon and the planets. This work was begun more than a century ago, and it has been repeated for the principal planets several times, so that now we have good tables of these bodies. In the case of the principal planets the labor of determining their orbits was facilitated by the approximate orbits handed down to us by the ancient astronomers; and also by the peculiar conditions of these orbits. For the most part the orbits are nearly circular; the planets move nearly in the same plane, and their motions are in the same direction. These are the conditions Laplace used as the foundation of the nebular hypothesis. With approximate values of the periods and motions, and under the other favoring conditions, it was not difficult to form tables of the planets. However the general problem of determining an orbit from three observations, which furnish the necessary and sufficient data, was not solved until about a century ago. The orbits of comets were first calculated with some precision. Attention was called to these bodies by their threatening aspects, and by the terror they inspired among people. It was therefore a happy duty of the astronomers to show that the comets also move in orbits around the Sun, and are subject to the same laws as the planets. This work was easier because the comets move nearly in parabolas, which are the simplest of the conic sections. Still the general problem of finding the six ele-

ments of an orbit from the six data given by three observations remained to be solved. The solution was given by Gauss a century ago in a very elegant manner. His book is a model, and one of the best ever written on theoretical astronomy. No better experience can be had for a student than to come in contact with such a book and with such an author. The solution of Laplace for the orbit of a comet is general, but demands more labor of computing than the method of Olbers, as arranged by Gauss. It is said by some writers that the method of Laplace is to be preferred because more than three observations can be used. In fact this is necessary in order to get good values of the derivatives of the longitudes and latitudes with respect to the time, but it leads to long and rather uncertain computations. Moreover it employs more data than are necessary, and thus is a departure from the mathematical theory of the problem. This method is ingenious, and by means of the derivatives it gives an interesting rule for judging of the distance of a comet from the earth by the curvature of its apparent path, but a trial shows that the method of Olbers is much shorter. Good preliminary orbits can now be computed for comets and planets without much labor. This, however, is only a beginning of the work of determining their actual motions. The planets act on each other and on the comets, and it is necessary to compute the result of these forces. Here again the conditions of our solar system furnish peculiar advantages. The great mass of the sun exerts such a superior force that the attractions of the planets are relatively small, so that the first orbits, computed by neglecting this interaction, are nearly correct. But the interactions of planets become important with the lapse of time, and the labor of computing these perturbations is very great. This work has been done repeatedly, and we now have good numerical values of the theories of the principal planets, from which tables can be made. Practically, therefore, this question appears to be well toward a final solution. But the whole story has not been told.

The planets, on account of their relative distances being great and because their figures are nearly spherical, can be considered as material particles and then the equations of motion are readily formed. In the case of n material particles acting on each other by the Newtonian law, and free from external action, we shall have $3n$ differential equations of motion, and $6n$ integrations are necessary for the complete solution. Of these only ten can be made, so that in the case of only three bodies there remain eight integrations that cannot be found. The early investigators soon obtained this result, and it is clearly stated by Lagrange and Laplace. The astronomer, therefore, is forced to have recourse to approximate methods. He begins with the problem of two bodies, the sun and a planet, and neglects the actions of the other planets. In this problem of two bodies the motions take place in a plane, and the integrations can all be made. Two constants are needed to fix the position of the plane of motion, and the four other constants pertaining to the equations in this plane are easily found. This solution is the starting point for finding the orbits of all the planets and comets. The mass of the sun is so overpowering that the solution of the problem of two bodies gives a good idea of the real orbits. Then the theory of the variation of the elements is introduced, an idea completely worked out into a practical form by Lagrange. The elements of the orbits are supposed to be continually changed by the attractions of

the other planets. By means of this theory, and the mathematical machinery given by Lagrange, which can be applied to a great variety of questions, the observations of the planets can be satisfied over long intervals of time. When this theory of the motions was carried out a century ago it appeared that the great problem of planetary motion was near a complete solution. But this solution depends on the use of series, which undergo integrations that may introduce small divisors. An examination of these series by Hansen, Poincaré and others indicates that some of them are not convergent. Hence the conclusions formerly drawn about the stability of our solar system are not trustworthy, and must be held in abeyance. But looking at the construction of our system, and considering the manner in which it was probably evolved, it appears to be stable. However the mathematical proof is wanting. In finding the general integrals of the motions of n bodies, the assumption that the bodies are particles gets rid of the motions of rotation. These motions are peculiar to each body, and are left for special consideration. In the case of the earth this motion is very important, since the reckoning of time, one of our fundamental conceptions, depends on this motion. Among the ten general integrals that can be found six belong to the progressive motion of the system of bodies. They show that the center of gravity of the system moves in a right line, and with uniform velocity. Accurate observations of the stars now extend over a century and a half, and we are beginning to see this result by the motion of our sun through space. So far the motion appears to be rectilinear and uniform, or the action of the stars is without influence. This is a matter that will be developed in the future. Three of the other general integrals belong to the theory of areas, and Laplace has drawn from them his theory of the invariable plane of the system. The remaining integral gives the equation of living force. The question of relative motion remains, and is the problem of theoretical astronomy. This has given rise to many beautiful mathematical investigations, and developments into series. But the modern researches have shown that we are not sure of our theoretical results obtained in this way, and we are thrown back on empirical methods. Perhaps the theories may be improved. It is to be hoped that the treatment of the differential equations may be made more general and complete. Efforts have been made in this direction by Newcomb and others, and especially by Gyldèn, but so far without much practical result.

The problem of three bodies was encountered by the mathematicians who followed Newton, and many efforts were made to solve it. These efforts continue, although the complete investigations of Lagrange appear to put the matter at rest. The only solutions found are of very special character. Laplace used one of these solutions to ridicule the doctrine of final causes. It was the custom to teach that the moon was made to give us light at night. Laplace showed by one of the special solutions that the actual conditions might be improved, and that we might have a full moon all the time. But his argument failed, since such a system is unstable and cannot exist in nature. But some of the efforts to obtain partial solutions have been more fruitful, and G. W. Hill has obtained elegant and useful results. These methods depend on assumed conditions that do not exist in nature, but are approximately true. The problem of two bodies is a case of this kind, and the

partial solutions may illustrate, but will not overcome, the fundamental difficulty.

The arrangement of our solar system is such that the distances of the planets from one another are very great with respect to their dimensions, and this facilitates very much the determination of their motions. Should two bodies approach very near each other the disturbing force might become great, even in the case of small masses. In the case of comets this condition happens in nature, and the comet may become a satellite of a planet, and the sun a disturbing body. In this way it is probable that comets and meteoric streams have been introduced into our solar system. We have here an interesting set of problems. This question is sometimes treated as one of statics, but since the bodies are in motion it belongs to dynamics. Further study may throw light on some relations between the asteroids and the periodical comets.

The great question of astronomy is the complete and rigorous test of the Newtonian law of gravitation. This law has represented observations so well during a century and a half that it is a general belief that the law will prove true for all time, and that it will be found to govern the motions of the stars as well as those of our solar system. The proof is cumulative and strong for this generality. It will be a wonderful result if this law is found rigorously true for all time and throughout the universe. Time is sure to bring severe tests to all theories. We know that the law of gravitation is modified in the motions of the matter that forms the tails of comets. There is an anomaly in the theory of Mercury which the law does not explain, and the motion of our moon is not yet represented by theory. The lunar theory is very complicated and difficult, but it does not seem probable that the defect in Hansen's theory will be found by recomputing the periodical coefficients, that have been already computed by many mathematicians and astronomers, and with good agreement by Hansen and Delaunay, by very different methods. Hansen was a computer of great skill, but he may have forced an agreement with observations, from 1750 to 1850, by using a coefficient of long period with an erroneous value. No doubt the error of this theory will be discovered. Back of all theories, however, remains the difficulty of solving the equations of motion so that the result can be applied with certainty over long periods of time. Until this is done we shall not be able to subject our law to a crucial test.

The constants that enter the theories of the planets and moon must be found from observations. In order to compare observations made at distant epochs, the motions of the planes of reference must be known with accuracy, and also the motion of our solar system in space. As the stars are our points of reference their positions and their proper motions must be studied with great care. This department of astronomy was brought to a high degree of order by the genius of Bessel, whose work forms an epoch in modern astronomy. The recent progress made in determining the positions of the stars in all parts of the heavens will be a great help to the investigations of the future. We must have observatories where accurate and continuous observations are made. Our country is well situated to supplement the work of Europe, and we hope it will never fail to add its contribution to the annals of astronomy. American astronomers should keep pace in the improvements for increasing the ease and accuracy of making observations. The spectroscope has given a new element in the motions of the stars, not to speak of the interesting physical re-

sults obtained by its use. Photography will give great aid in determining the relative positions of the stars and in forming maps of the heavens. All new methods, however, will need examination and criticism, since they bring new sources of error. Fifty years ago it was thought the chronograph would increase very much the accuracy of right ascensions. It has not done this directly to any great extent, but it has increased the ease and rapidity of observing. We must remember that astronomical results finally depend on meridian observations, and that it is the duty of astronomers to make these continuous from generation to generation. In this way we shall gain the powerful influence of time to help control and solve our problems. There is one point where a reform may be needed from the dead weight of the large and expanding volumes sent forth by observatories and scientific institutions. The desire for publication is great, but the results should be well discussed and arranged, so that the printing may be shortened. Otherwise our publications may become burdensome, and when they are piled up in libraries some future Caliph Omar may be tempted to burn them. Even mathematics appears to labor under a similar oppression, and much of its printed matter may be destined to moulder to useless dust.

In the not distant future stellar astronomy will become a great and interesting field of research. The data for the motions of the stars are becoming better known, but these motions are slow, and the astronomer of to-day looks with envy on the astronomer of a thousand years hence, when time will have developed these motions. Much may be done by the steady and careful work of observation and discussion, and the accumulation of accurate data. Here each one of us can add his mite. But the great steps of progress in science have come from the efforts of individuals. Schools and universities help forward knowledge by giving to many students opportunities to learn the present conditions, and from them some genius like Lagrange or Gauss may come forth to solve hard questions, and to break the paths for future progress. This is about all the schools can do. We need a body of men who can give their lives to quiet and continuous study. When the young Laplace was helped to a position where he could devote his life to research D'Alembert did more for the progress of astronomy than all the universities of Europe.

One needs only to glance at history to see how useful astronomy has been in the life of the world. It has wonderfully enlarged the universe, and widened the views of men. It shows how law and order pervade the world in which we live; and by the knowledge it has disseminated and by its predictions it has banished many superstitions and fears. The sciences will continue to grow; and they will exert the same influence. The erroneous and dogmatic assertions of men will be pushed aside. In our new country the energies of the people are devoted chiefly to commercial and political ends, but wealth is accumulating, leisure and opportunity will come, and we may look forward to a great development of scientific activity. We must be patient. Men do not change much from generation to generation. Nations that have spent centuries in robbery and pillage retain their dispositions and make it necessary for other nations to stand armed. No one knows when a specious plea for extending the area of civilization may be put forth, or when some fanatic may see the hand of God beckoning him to seize a country. The progress of science and invention will

render it more difficult for such people to execute their designs. A century hence it may be impossible for brutal power, however rich and great, to destroy a resolute people. It is in this direction that we may look for international harmony and peace, simply because science will make war too dangerous and too costly.

The influence of the sciences in bringing men of different nationalities into harmony is great. This is done largely by the common languages that are formed in each science. In mathematics the language is so well formed and generally adopted that mathematicians all over the world have no trouble in understanding one another. It may be difficult to read Russian, but every one can read the formulas of Tchebitchef and Lobaschewsky. In astronomy the common language is nearly as well established, so that there is little difficulty in understanding the astronomy of different nations. A similar process is going on in chemistry, botany and in the other sciences. When men are striving for the discovery of truth in its various manifestations, they learn that it is by correcting the mistakes of preceding investigators that progress is made, and they have charity for criticism. Hence persecution for difference of opinion becomes an absurdity. The labors of scientific men are forming a great body of doctrine that can be appealed to with confidence in all countries. Such labors bring people together, and tend to break down national barriers and restrictions. The scientific creed is constantly growing and expanding, and we have no fears, but rejoice at its growth. We need no consistory of bishops, nor synod of ministers, to tell us what to believe. Everything is open to investigation and criticism.

In our country we have one of the greatest theaters for national life that the world has ever seen. Stretching three thousand miles from ocean to ocean, and covering the rich valleys of the great rivers, we have a land of immense resources. Here is a vast field for scientific work of various kinds. No doubt the men of the future will be competent to solve the problems that will arise. Let us hope that our national character will be just and humane, and that we may depart from the old custom of robbing and devouring weak peoples. Any one who saw the confusion and waste in this city in 1862 might well have despaired of the Republic; and he who saw the armies of Grant and Sherman pass through the city in 1865 felt that he need fear no foreign foe: neither French emperor, nor English nobleman nor the sneers of Carlyle. To destroy a democracy by external force the blows must be quick and hard, because its power of recuperation is great. The danger will come from internal forces produced by false political and social theories, since we offer such a great field for the action of charlatans. Our schools and colleges send forth every year many educated people, and it is sometimes disheartening to see how little influence these people have in public life. Those who are trained in the humanities and churches ought to be humane in dealing with other people, ready to meet great emergencies and powerful to control bad tendencies in national affairs. But this is rarely the case. On the other hand the most unscrupulous apologists and persecutors have been educated men, and the heroes of humanity have come from the common people. This anomaly points to something wrong in the system of education, which should disappear. The increase and teaching of scientific ideas will be the best means of establishing simple and natural rules of life. Nature, and science her interpreter, teach us to be

honest and true, and they lead us to the Golden Rule. Asaph Hall.

POPULAR SCIENCE.*

Ladies and Gentlemen: Five years ago I prepared a sketch of an address which I expected to deliver as retiring president of the Iowa Academy of Science. I was not able to deliver the address, however, on account of enforced absence from the Des Moines meeting of the Academy at Christmas time, 1897. It was my intention in that address to speak in terms of commendation of some of the ideas advanced by Professor Woodrow Wilson in his then recent address given on the occasion of the Sesquicentennial celebration of Princeton University. Professor Wilson's recent promotion to the presidency of Princeton University has called his Sesquicentennial address again to our minds, and it seems to me that I may very properly say now what I had intended to say in 1897, especially inasmuch as no one, speaking for science, has expressed any degree of sympathy with President Wilson's point of view. I hope to make my meaning so clear and definite as to render it unnecessary for me to limit or qualify my general expression of sympathy with Professor Wilson; although the words he has used in his Sesquicentennial address are certainly open to an interpretation which no seriously minded man of science could possibly accept.

In order that we may enter upon this subject with some degree of mutual understanding, I think it is necessary to quote President Wilson at some length. He says, "I am much mistaken if the scientific spirit of the age is not doing us a great disservice, working in us a certain great degeneracy. Science has bred in us a spirit of experiment and a contempt for the past, * * *" yet "I have no indictment against what science has done: I have only a warning to utter against the atmosphere which has stolen from our laboratories into lecture rooms and into the general air of the world at large. * * *" Science "has driven mystery out of the universe; it has made malleable stuff out of the hard world and laid it out in its elements upon the table of every class room. Its own masters have known its limitations; they have stopped short at the confines of the physical universe; they have declined to reckon with spirit or with the stuffs of the mind, have eschewed sense and confined themselves to sensation. But their work has been so stupendous that all other men of all other studies have been set staring at their methods, imitating their ways of thought, ogling their results." "Let me say once more, this is not the fault of the scientist, he has done his work with an intelligence and success which cannot be too much admired. It is the work of the noxious and intoxicating gas, which has somehow got into the lungs of the rest of us from out of the crevices of his workshops—a gas it would seem, which forms only in the outer air, and where men do not know the right use of their lungs. * * *" "We have not given science too big a place in our education, but we have made a perilous mistake in giving it too great a preponderance in method over every other branch of study. We must make the humanities human again; we must recall what manner of men we are; must turn back once more to the region of practicable ideals. * * *" "I should fear nothing," says President Wilson, "better than utter destruction from a revolution conceived and led in the scientific spirit."

The chief obstacle to me in my attempt to reach a satisfactory appreciation of President Wilson's point of view lies in his apparently loose and unguarded use

* Address of the Chairman of Section B and Vice-President of the American Association for the Advancement of Science, read at the Washington meeting, December 29, 1902.

of the term 'scientific spirit.' If he means by it that humble spirit of inquiry based upon systematic methods of analysis which are really applicable to the nature of the inquiry, I certainly can not agree with him that it can do any disservice or that it would be anything but a basis of hope as the ruling element in a revolution. I do not believe that President Wilson entertains any such idea. If he means, however, to signify by 'scientific spirit' that widespread and portentous 'neglect of the essential qualities in things,' I most certainly approve his meaning and share his feelings of distress, although I disapprove his mode of expression.

Scientific men are of course not entirely free from this neglect of the essential qualities in things, but I think that the chief neglect lies in the general popular imagination, and I believe that the growth of modern science and the resulting transformations of our material world, have brought upon us an acute and distressing manifestation of it. Inasmuch as I intend to speak to you mainly of the nature and extent of the influence of scientific work on the popular imagination, I may claim to speak on popular science.

We can not discuss intelligently any subordinate manifestation of science until we come to some mutual understanding as to what science itself is; but I must confess that I do not like to go to the extent of defining a thing which, in my own mind at least, is so severely plain and humble. I do not know how you feel, but for my part I am sick of this disgusting din which has been increasing for a hundred years in canting praise of science, a din which I can most easily specify to your perception by saying that my reluctance to define science is chiefly the fear that a pack of popular idiots will rise up with indiscriminate shouting and say—you know, of course, that I have endless choice of ridiculous sayings of influential men in needless and foolish praise of science to quote from! Science does not need praise, nor does work need praise; they both need plain wages. I think it is time to urge a definition of science which will help to purge the popular imagination. Science is the spirit of work. I do not mean the spirit of a man who works, but I do mean simply that science has to do solely with the increasing efficacy of the sweaty labor of this world. I am little disposed to argue what many of you may be inclined to think an undue narrowness in this definition, but I assure you that it is wide enough for me. 'An affected thinker,' says Ruskin, 'who supposes his thinking of any other importance than as it tends to work is about the vainest kind of person that can be found' among busy men.

My own knowledge of science rests partly on anticipation and partly on a college and university experience more than usually varied, and I am convinced that science is 'primarily concerned with the making of breeches,' although, of course, you know and I know many things not now applicable to that useful, or in some cases it may be useless, business. Perhaps one who is chiefly engaged in technical education is prone to accept that practical view, yet one should not, I think, attempt to escape the evidence of one's experience, the less so, indeed, the more intimately his experience is related to practical affairs, and in any case one should only strive against exaggerated inference and extravagant conclusion.

I trust that the granting of my contention as to the severe and unpretentious homeliness of science may not divest it in your minds of a bloom which you deem essential to your interest in it; but however that may be, an understanding of what I have to say demands that much of you.

I hesitate to accept President Wilson's ideal of the perfect place of learning of which he says: 'Calm Science [is] seated there, recluse, ascetic, like a nun, not knowing that the world passes, not caring, if truth but come in answer to her prayer; and Literature walking within her open doors, in quiet chambers, with men of olden time, and calm voices infinitely sweet,' for I fear that President Wilson assumes that the spirit of science is the same as the spirit of literature which is no less a grievous error than to assume that the spirit of literature is the same as the spirit of science. I can not think of science as 'recluse, ascetic, like a nun'; but unquestionably the true seat of learning is a place apart from the world, hedged about by virtue, intrenched in grace and beauty like a woman's womb, its air pure and wholesome with the breath of faith, and looking to heaven for the confirmation of its hope.

I am inclined to look upon science as a servant and I have no sympathy for that state of mind which is exemplified by two extreme types; the man of alleged general culture who has so far forgotten his manhood as to be lost in vacant, staring wonder at the material results of modern science, but who remains in either lazy or stupid ignorance of the underlying method, and the specialist who sighs for those good old days when one man's mind might compass the entire range of scientific activity. This second type is a man who errs mainly in false humility and I am reminded in this connection of the character of Wagner in Goethe's Faust, second part, who humiliates himself before a creature of his own devising, the Homonculus. I take it to be self-evident that science can never transcend the intellectual grasp of a single man. Of course we must remember that as in case of a large industrial establishment there are many details which cannot be carried forward by the superintendent alone, so in science there are many special details which cannot be carried forward by one person, but if we consider rightly, I think it must appear that these details are essentially not intellectual.

Concerning those whose interest in science is based upon its results, I think you will agree with me that no intelligent interest can be so founded. Everything that appears in the name of science in our newspapers and magazines relates only to results. Have any of you seen in our newspapers or popular magazines any detailed description of the principles and methods used by Marconi in his wireless telegraphy? I think you have not, and yet we know too well that there is not a newspaper reader in the country but imagines he has an idea of wireless telegraphy simply because he has read that Marconi has signaled across the Atlantic Ocean!

I am somewhat intimately connected with the teaching of electrical engineering, more intimately, perhaps, than my chief interests warrant, and I frequently have occasion to speak with non-technical men respecting this subject. There are, indeed, many plain men who keep their senses when they speak of the developments in applied electricity and who talk with some degree of rudimentary intelligence concerning these things, but there are many, very many, more who seem to imagine that the glad comfort with which they ride in a trolley car constitutes an intelligent interest in science and has an intellectual quality!

True interest in science begins when one gets an idea into one's head and sees its firm and unequivocal application to external fact, and the characteristic feature of the study of science is a *determining objective constraint upon the processes of the mind.* I am surprised that this one important feature of science study is never

mentioned in the many estimates that have been made of the value of science study in education, for as a matter of fact that complete definiteness which is usually urged as the characteristic feature of science study is the fundamental condition of every psychological process; you say this or you say that, you go or you do not go; and the psychological processes which play in the study of science do not differ from other psychological processes in this respect, absolutely not at all.

Let me illustrate this objective character of science study by an example which happens also to illustrate an error which I suppose many of you entertain. What is the definition of the mass of a body? The careless and imaginative definition which is usually given is that 'the mass of a body is the quantity of matter the body contains.' I suppose that definition satisfies many of you, but it does not satisfy me. All our notions of length and angle take their rise in and are fixed or defined by those fundamental geometric operations of congruence. The real definition of mass is no less a physical operation, the verbal definition is the briefest possible specification of this operation and it can be nothing else, the result of this operation on a given body is an invariant number, and by a feat of the imagination we conceive this invariant number to be a measure of the quantity of matter the body contains. Ask a farmer's boy how he would define or set the boundaries to a cow pasture, explaining to him that you seek real practical information, and I think he could only answer, by building a fence around it! Most of our definitions in physics which apply to sensible things are necessarily applied to ideally simplified conditions which can not be feasibly realized as actual operations, all for the sake of simplicity and directness of statement, and the consequence is, I think, that many of us lose sight of the fact that these definitions are in reality operations.

I sometimes think that no popular scientific writings should be tolerated which do not introduce the reader to some appreciation of the exacting requirements of successful work. Some of Jules Verne's stories, for example, are peculiarly faulty in this respect, and these stories, and many others like them, are largely responsible, in my opinion, for the widespread fancied interest in science on the part of those who really care only for its immediate results. Most persons are fascinated by Jules Verne's care-less trip to the moon and by the easy improvidence of his ten thousand leagues under the sea.

A short time ago I had occasion to review a little book in the pages of SCIENCE, and I found therein an opportunity to briefly state what in my mind is a more serious perversion of science than that which is presented by those whose fancied interest in it is based on its results, and who, poor fools, invest in Keeley motors and sea gold companies because, forsooth, the desired result is so clearly evident. Surely one can not hold the 'scientific spirit' accountable for 'great degeneracies' like these. The book in question purports to treat of the atomic theory, it is prefaced by an introduction by a professor in the University of Chicago, and it deserves a place in DeMorgan's 'Budget of Paradoxes.' I mentioned in my review, to begin with, a list of headings to serve to indicate to the general reader the present scope of the atomic theory; the atomic theory of gases, the theory of crystal structure, the molecular theory of elasticity, the electro-atomic theory of radiation, the corpuscular theory of the electric discharge and of the electric current, stereo-chemistry, and the like, and I expressed it as my conviction that neither the author nor

his introducer knew even a little of these things.

When I take up a book like the one under consideration I am always impelled to ask myself the question, What are atoms? although in studying ordinary books on physical science the question never forcibly occurs to me. In so far as we have anything really to do with atoms, I believe they are mere logical constructions. Bacon long ago listed in his quaint way the things which seemed to him most needful for the advancement of learning. Among other things he mentioned 'A New Engine, or a help to the mind corresponding to tools for the hand,' and I think that the greatest achievement of the nineteenth century in the physical sciences is the realization of Bacon's idea in a great body of useful theory. Helmholtz says: 'It is a great advantage for the sure understanding of abstractions if one seeks to make of them the most concrete possible pictures, even when the doing so brings in many an assumption that is not exactly necessary.' Just how much of this useful theory is to become the common property of all men it is impossible to say. For the theory is by no means fixed and may not be for a century to come, and no one but the most determined specialist can be expected to appropriate and use the more complex theories which depend upon the keenest mechanical sense, the sharpest algebraic faculty, the strongest geometrical imagination, and the most devoted study; but there is a great and growing body of simple conception and theory which can and does represent to the understanding a vast array of fact.

This New Engine, as Bacon calls it, is a necessity to every man in so far as its state of perfection and the limited opportunity for education permits, and on these two conditions no one need fear any serious clogging of men's minds by it. Many scientists do not, however, fully realize, I think, that the great majority of men do not have and should not have any interest, or at least they should not expend their energies, in those border regions of science where uncertainty and obscurity necessarily and prevailingly obtain. The failure of a specialist to realize the remoteness of his work from legitimate popular interest often results in his endeavor to capture the popular imagination by sensational announcements of which we see only too many examples. The fact is that specialization in science requires a degree of renunciation and to the extent that this requirement is not met by scientists they do a disservice to their fellow men. I believe indeed that no man can do honest and effective work as a specialist and fail to meet this fundamental requirement; and the disservice that accrues when he attempts to evade it is illustrated most distressingly by that would-be electro-scientist who has recently telegraphed to Mars!

A career in which one could come into sympathetic touch with great numbers of men would be very satisfactory to most of us, no doubt, but the career of the scientific specialist is not such, and I can not refrain from stating it as my conviction that a sufficiently guarded appropriation of, say, ten per cent. of the income of the Carnegie endowment for furthering the personal intercourse of scientific specialists would be productive of greater results by far than could possibly be effected by the expenditure of the remaining ninety per cent. in any other way whatever. I say this more particularly from the point of view of the western man.

I think, with President Wilson that scientists have, as a rule, recognized the limitations of their work, and I certainly think, also, that other men err in attribu-

ting to science too great an extensity and in failing to reach any just appreciation of the intensity of science. Every one should know that a specialist's idea of a thing, such as a gas, an electric current, or a beam of light, comes very near to being a working model of the thing. The elements out of which such models are made are purely notional, and although the specialist habitually speaks of them in objective terms for the sake of concreteness and clearness, it is of the utmost importance that the thought be chiefly directed to the physical facts which are represented and not to the models themselves. 'Our method,' says Bacon, 'is continually to dwell among things soberly, without abstracting or setting the mind farther from them than makes their images meet,' and 'The capital precept for the whole undertaking is that the eye of the mind be never taken off from things themselves, but receive their images as they truly are, and God forbid that we should ever offer the dreams of fancy for a model of the world.'

There is a tendency among reflecting men to confuse the boundaries between our logical constructions and the objective realms which they represent to the understanding. Münsterberg thinks that this is the gravest danger of our time. I do not fully agree with this, but I do agree with President Wilson in seeing in this confusion of boundaries the effects of a noxious gas which has somehow got into the lungs of other men from out of the crevices of our workshops, a gas, it would seem, which forms only in the outer air and where men do not know the right use of their lungs.

This confusion of boundaries is, to my mind, a new species of idolatry. The old idolatry is the worship of form, and this new idolatry is that contemplation of our logical constructions which despises objective constraint. Now, I can not see that we as scientists are in any degree responsible for this disservice, this working of a great degeneracy among men, but as individuals I think most of us are guilty of more or less frequent and flagrant lapses of that submission to objective constraint which is the very essence of moral quality in scientific work.

An amusing collection of instances of this new idolatry, which we all know is not so very new after all, is given by DeMorgan in his 'Budget of Paradoxes.' There are many more of these paradoxes, to use DeMorgan's word for those unconstrained flights of the scientific imagination, in the mathematical and physical sciences than in biology. The explanation of this fact is, I think, that the logical structures of those sciences are to a great extent concrete in character so that even strong minds may lose sight of the boundaries between the realms of the mind and the realms of objective reality. The wide difference between the logical structures of physics and of biology may be further illustrated if I mention that I have long been impressed with the fact that the most satisfactory specialist to talk with is the biologist. His knowledge is not represented to his understanding by a mathematical-mechanical system of conceptions, but it approaches art in its close association with external form. Conversation with a physicist is, however, very like looking into the mechanism of a Mergenthaler type-casting machine, with the machine out of sight, a thing which is feasible enough among designers and builders, but scarcely a satisfactory basis for the flow of thought when one party in the conversation happens to be unfamiliar with and perhaps not interested in the mechanism in question.

Having so far expressed a degree of sym-

pathy with President Wilson in the distress which some of the results of science, direct or indirect, have given him, I wish to say that giving the words of his sesquicentennial address their most sinister interpretation a modern man would infer that President Wilson is inclined to turn back to the hope of a revival of classical and cloistered erudition as the chief end of learning. Now, I think that many of us feel that science itself is threatened by just this sort of thing in its own field. Many of us in fact know so much of the partial knowledges that have been reached during the century that we are deterred from effective work. 'We advise all men,' says Bacon, ' to think of the true ends of knowledge, and that they endeavor not after it for curiosity, contention, or the sake of despising others, nor yet for profit, reputation, power, or any such inferior consideration, but solely for the occasions and uses of life.'

Above all I believe it to be in general a perverting thing to use the elements and results of science as a basis of metaphysical speculation. 'I believe,' with Ruskin, 'that Metaphysicians and Philosophers are, on the whole, the greatest troubles the world has got to deal with; and that, while a tyrant or bad man is of some use in teaching people submission or indignation, and a thoroughly idle man is only harmful in setting an idle example, and communicating to other lazy people his own lazy misunderstandings, busy metaphysicians are always entangling good and active people and weaving cobwebs among the finest wheels of the world's business; and are as much as possible by all prudent persons to be brushed aside like spiders.'

There is, of course, a legitimate sphere of scientific speculation of a certain kind, but the purely suggestive and highly tentative efforts in this line should not be confused with the more substantial work of science, and this is precisely what happens in the popular imagination. The majority of men do not appreciate the difference between a discussion of the motion of stars in the line of sight based upon spectroscopic measurements and a discussion of the habitation of Mars based on nothing at all! Idle speculation is the last infirmity of strong minds, but it is certainly the first infirmity of weak ones, and popular science is, I think, primarily speculation.

The extent to which some of our elementary text-books in physics indulge in weak phases of speculation is very surprising to me for in this connection it is absolutely out of place and entirely misleading. What do you think, for example, of the following quotation from Maxwell as a help to clear up an inadequate definition of energy in a secondary school book in physics? "We are acquainted with matter only as that which may have energy imparted to it from other matter, and which may in its turn communicate its energy to other matter. Energy, on the other hand, we know only as that which in all natural phenomena is continually passing from one portion of matter to another." What do you think of the following from an elementary English text-book? "The fundamental property of matter, which distinguishes it from the only other real thing in the universe, is inertia. * * * We are now in a position to give one or two provisional definitions of matter—provisional because we cannot yet say, possibly may never be able to say, what matter really is. It may be defined in terms of any of its distinctive characteristics. We may say that matter is that which possesses inertia, or again since we have no knowledge of energy except in association with matter, we may assert that matter is the Vehicle of Energy." I

wonder if any of you really doubt that every notion in physics, definite or indefinite, is associated with and derived from a physical operation, and that absolutely the only way to teach physics to young men is to direct their attention to that marvelous series of determining operations which bring to light those one-to-one-correspondences which constitute the abstract facts of physical science. If you do, I am bound to say I do not think much of your knowledge or teaching of physics. I think that the sickliest notion of physics, even if a student gets it, is that it is 'the science of masses, molecules and the ether.' And I think that the healthiest notion, even if a student does not wholly get it, is that physics is the science of the ways of taking hold of bodies and pushing them!

W. S. FRANKLIN.

*INCOMPLETE OBSERVATIONS.**

IN scientific literature many observations are recorded which, from the experimental proof offered, have been generally recognized as true, but which may be classed as *incomplete*, owing to the fact that the methods of investigation employed destroyed conditions that were later found to exist, or that subsequent discoveries modified the conclusions reached at the time of the original investigation.

As an illustration of this proposition the theories of alcoholic fermentation may be cited. The members of Section C will readily recall the long and bitter controversy which was waged between the two great masters, Liebig and Pasteur, and their respective adherents as to the true cause of this phenomenon.

It is interesting at this time, in the light of recent observations, to compare the two opposing theories.

According to Liebig alcoholic fermentation is caused by the decomposition of complicated nitrogenous bodies designated by him as putrescible material, and the molecular disturbance thereby produced is imparted to the fermenticible substance, sugar, and breaks it up into simpler bodies, alcohol and carbon dioxide.

The vitalistic theory, revived by Pasteur and brought to general recognition by his masterly and convincing experiments, teaches that alcoholic fermentation takes place only in the presence of a living microorganism known as the yeast plant, and that the phenomenon of fermentation is intimately connected with the life process of this organism. The most convincing proof in support of the vitalistic theory was furnished by Pasteur in his methods of preventing fermentation and allied phenomena by simply heating perishable bodies to a temperature high enough to kill the living germs. In the case of acetic acid fermentation he showed that a temperature of 60° was sufficient to destroy the vinegar plant. At this temperature, he argued, the nitrogenous bodies, which Liebig claimed as the actual ferments, would remain intact. In spite of this, however, he showed that further fermentation was completely arrested so long as living germs were excluded.

Although the work of Pasteur was of the greatest importance to science and humanity, and his experimental evidence for the establishment of the vitalistic theory of fermentation was of the highest order, yet to the minds of many it was never entirely clear that the rival theory was completely overthrown. For a long time, however, the vitalistic theory had clear sailing. But the observations which led to its adoption remained incomplete until a few years

* Address of the Chairman of Section C and Vice-President of the American Association for the Advancement of Science, read at the Washington meeting, December 29, 1902.

ago Buchner startled the scientific world by the announcement that he had produced alcoholic fermentation without the presence of a single living germ. By simply mixing the extract, obtained by strong pressure from brewer's yeast, containing nothing but *dead* organic matter, he caused a solution of grape sugar to ferment, and, in fact, much more rapidly than if the yeast itself had been employed. Not only this—Buchner showed, furthermore, that the activity of this extract was completely destroyed at a temperature below that required to kill the yeast plant. This is the important point in Buchner's observations, because it was the failure to recognize this fact by Pasteur and his adherents that helped, more than anything else, to give the death blow to Liebig's theory. It is true that Liebig at first did not regard his putrescible matter or ferments as a product of the ever-present organisms, and it is also true that in Buchner's extract it is the enzyme of the yeast plant which produces the molecular disturbance that causes the grape sugar to break up into alcohol and carbon dioxide; yet it is gratifying to all those who were students of the great master to learn that, in the main, his attitude toward the process of fermentation has been finally vindicated.

It was the desire of the writer to discuss on this occasion some subject related to that branch of chemistry with which he is at present identified, and for this purpose the investigations in regard to assimilation of free nitrogen by plants were selected for consideration, since this question belongs in the category of 'incomplete observations.'

The importance to agriculture of knowing whether plants were capable of assimilating the free nitrogen of the air was impressed upon the minds of the early investigators of the subject of plant nutrition, because if this element in the free state so liberally supplied by nature should be found to be available as plant food, then it would fall into the same class with carbon, hydrogen and oxygen, which furnish the bulk of all vegetable matter, and about whose source the farmer need have no concern. In the early fifties the French chemist, Boussingault, conducted his memorable experiments with various kinds of plants in order to settle this question. His apparatus consisted of a large glass, one-necked globe, into which he introduced a sufficient quantity of soil freed from nitrogen compounds by ignition. In this soil he planted a certain number of seeds, supplied a sufficient amount of water and then hermetically sealed into the neck of the globe a smaller one filled with carbon dioxide. Under this arrangement the seeds were allowed to germinate and the plants to grow. After a period of several weeks the plants with their roots were carefully removed, dried, weighed and the nitrogen determined. He then determined the nitrogen in a like number of seeds themselves and compared the results. Out of fourteen experiments with various kinds of plants, including the legumes, he found in eleven cases a minus quantity of nitrogen in the plants and in the other three a small plus quantity. The latter results, however, he considered within the limits of errors of observation. His conclusion, therefore, was that the free nitrogen was not available plant food.

At the same time another French chemist, Ville, investigated this problem. His experiments were made on a somewhat larger scale, his apparatus consisting of iron sash filled with glass. Ville uniformly found a marked increase in the content of nitrogen of the plants over that of the seeds, and since nitrogen compounds had been excluded during the time of his experiments, he concluded that the source of

this increase was necessarily the free nitrogen of the air. His objection to Boussingault's conclusions was based upon the claim that, in the confined space in which the plants were forced to grow, their natural development was hindered.

Ville's criticism led Boussingault to repeat his experiments. In order to meet the former's objection to the limited amount of air in which the plants were forced to vegetate, he substituted a three-necked globe for the one employed before. By using an aspirator the air in this globe could be continually renewed, after passing it through a series of Wolf's bottles with the proper solutions to free it from nitrogen compounds. The results of this second series of experiments fully corroborated his former conclusions.

A committee appointed by the French Academy of Sciences to investigate the methods employed by Boussingault and Ville held that, in the latter's experiments, the introduction of nitrogen compounds was not excluded, and, therefore, pronounced in favor of Boussingault. If any doubt had remained in regard to the correctness of Boussingault's conclusions it was dispelled a few years later by the labors of Laws, Gilbert and Pugh. These investigators repeated the experiments of Boussingault with expensive and improved apparatus. Their work was performed with the greatest care and nicety, and their results fully vindicated Boussingault in the position he had taken.

The experimental evidence thus produced in favor of the proposition that the free nitrogen of the air was not available for vegetable growth was so clear and convincing that it was readily accepted by all, with the exception of one man. This man was George Ville, of France.

During all the time in which this opinion prevailed, he alone remained firm in the belief that his observations were true, and that plants could assimilate free nitrogen.

That plants can not assimilate free nitrogen directly was established by those early investigators without a doubt. On the other hand, it is now equally well established that free nitrogen does become available as plant food and plays an important part in vegetable production.

Evidently, therefore, the early investigations must have been incomplete, and at this distant day it is not difficult to point out wherein they were defective. Boussingault and Ville, as well as Laws, Gilbert and Pugh, regarded the soil as a mixture of mineral matter and humus. They had no conception of the fact that it was the home of a world of living microorganisms, which in a variety of ways are silently and incessantly active in the transformation of matter essential to vegetable growth. Hence it is but natural that, in preparation of soil free from nitrogen compounds of all kinds, they should, what any chemist under like conditions would do, subject their soil to an intense heat.

Notwithstanding the prominence of these investigators and the general recognition accorded to their conclusions, further work in this connection was at most only retarded but not entirely abandoned. Facts known at that time, and new observations gradually made in studying the soil in all of its phases, began to point in the opposite direction.

With the discovery of Berthelot, that the fixation of free nitrogen took place through the instrumentality of silent electrical discharges in the soil, were associated the manifold effects upon matter, shown to be due to the action of bacterial life. These latter discoveries may be divided into two groups:

1. Those showing the independent action of bacteria in the soil in causing fermenta-

tion, nitrification, denitrification and fixation of free nitrogen.

2. Those showing the fixation of free nitrogen by microbes in symbiotic relation to higher plants.

The first group of observations including the fixation of free nitrogen in the soil as pointed out by Berthelot and others is of great importance to agriculture, but the amount of available nitrogenous plant food produced by the various processes discovered is not sufficient for the demands of intensive farming. The truth of this statement can be inferred from the fact that, in addition to the enormous amount of nitrogenous material obtained from domestic and industrial sources, as well as from the extensive deposits of guano, there are, at the present time, about one million tons of Chili saltpeter employed annually by farmers the world over to maintain partially the fertility of their fields.

The second group of observations are of greater interest to agriculture, since they point out the way of securing from the free nitrogen of the air an ample amount of combined nitrogen to meet all the requirements of intensive farming. They make the farmer independent of the natural deposits of nitrogenous fertilizers, and furnish him the means of preventing his helplessness, in case these sources of plant food should become exhausted or otherwise unavailable.

From the time of the ancients down to the present day the legumes, especially the clovers, have occupied a unique position among agricultural crops. The beneficial effects of a crop of clover upon subsequent grain crops was a matter of practical experience in ancient and mediæval times, and this empirical knowledge was applied more or less in the practice of agriculture during those periods, as well as in modern times. When the science of chemistry began to shed light upon the production of vegetable matter, and showed the relation which plants, soil and air bore to each other, and especially that certain elements contained in the soil and air were essential to vegetable growth, the peculiar properties of the legumes received early attention. It was soon learned that the leguminous plants were preeminently nitrogen-gatherers. Having accepted the conclusions of Boussingault in regard to free nitrogen as true, the teachers of agricultural chemistry were forced to explain this property of the leguminous plants in various ways. Besides the empirical observations, already alluded to, many comparative experiments were made which showed the beneficial effects of legumes on subsequent grain crops. As an example the experiment of von Wulffen may be cited. One half of a certain field was allowed to remain in bare fallow, while the other half was sown to yellow lupines. After the lupines had fully developed the whole field was plowed and sown to rye. The yield of the two halves was determined separately with the following results:

	Grain.	Straw.
After lupines	532.5 lb	1,072 lb
After bare fallow	322 lb	656.5 lb

Here was a total increase in grain and straw of 626 pounds on that half of the field which had been sown to lupines, while nothing from without had been added to it except sixty pounds of lupine seed. The results of this experiment also show, what was claimed above, that the independent, bacterial activity of the bare fallow fell far short of producing sufficient available plant food for a full crop of rye.

In seeking an explanation for this effect of the legumes, Boussingault determined the amount of refuse, *i. e.*, stubble and roots, left in the soil by various crops. For this purpose he had the roots, etc., collected

from measured plots of fields from which the crops had been harvested. His results are given in kilos per hectare and refer to dry matter. The nitrogen of the refuse was also determined. His figures are given in the following table:

	Crop.	Refuse.	Nitrogen of Refuse.
Wheat	1,002	518	2.1
Oats	1,608	650	2.6
Clover	1,975	1,547	27.9

If it be considered that the essential ash ingredients of plant food are equally high in the clover refuse, it will be seen that the manurial value of the clover refuse is out of all proportion to that of the two cereals, and consequently that clover must be a better forerunner for a grain crop than a grain crop itself. But Boussingault did not stop here. He also collected the refuse matter, roots and leaves from a crop of mangolds, and found that not only the dry matter, but also the nitrogen contained therein, was in excess of that of the clover. Here was a dilemma; for it was well known that, compared to legumes, root crops were poor forerunners for grain crops. The explanation for this apparent contradiction was found in extensive experiments made at Rothamstead. Laws and Gilbert raised root crops on the same field for years in succession without the application of manures, and found that they rapidly exhausted the surface soil. On the other hand, they showed that with clover, even after the removal of a highly nitrogenous crop, the soil was left richer in nitrogen than it was before. It is but fair to state in this connection that other investigators found much larger yields with clover than Boussingault. Thus, to take the other extreme, Heiden obtained from measured plots of clover, after it had become fully ripe, and by removing the whole aerial portion of the crop, the following results, expressed in kilos per hectare:

	Aerial Portion.	Roots.
Dry matter	14,548	8,469.6
Nitrogen	381.5	275.3

Laws and Gilbert, Heiden, and in fact all who investigated this subject explained this large accumulation of nitrogen principally by the assumption that clover, on account of its deep roots, had the power, in a marked degree, of obtaining a large portion of its food from the subsoil and bringing it to the surface. Furthermore, it was assumed that on account of the great leaf surface of clover, its more succulent nature and its longer period of growth, it was capable of collecting more ammonia from the air than was the case with grasses and cereals. Another peculiarity which the legumes were thought to possess was their ability to assimilate, in a higher degree than other crops, the reserve nitrogen of the soil. This assumption would explain, of course, why these plants should make a luxuriant growth on soils on which, for lack of available nitrogen, other crops failed to make a good stand, but it would not throw any light upon the fact, established by general observation, that the total fixed nitrogen of the soil was so materially increased.

It may be truthfully said that all these explanations taken together were not entirely satisfactory to those who were engaged in the teaching of agricultural chemistry, but, in short, this was the status of the nitrogen question for a generation or more, when Hellriegel announced before the section of agricultural chemists of the German Association of Men of Science and Physicians, at their meeting in 1886, that the leguminous plants could assimilate the free nitrogen of the air, and that this assimilation was intimately connected with the nodules appearing upon the roots of

these plants. The hearty applause with which this announcement was received at the meeting, and the widespread and spontaneous interest which it awakened all over the world, showed that it came as a relief to agricultural chemists and vegetable physiologists in general. The report of Hellriegel was based upon observations and experiments made during the four preceding years. He had been appointed jointly with Wilfarth as referee on the subject of nitrogen assimilation by plants. The experiments were made in pots containing four kilos of recently ignited sand, to which the proper amount of mineral plant food, free from combined nitrogen, had been added. The main points established were as follows:

1. When no combined nitrogen was added to the artificial soil the acquisition of nitrogen over that contained in the seeds was naught. This was true for all kinds of plants, including the legumes.

2. The development of all kinds of plants and the acquisition of nitrogen were in direct proportion to the amount of combined nitrogen added.

3. When a small quantity of natural soil, or of an aqueous infusion of such soil, was added to the contents of the pots and no other combined nitrogen introduced, the graminaceous plants, as well as some other families of plants, died of nitrogen starvation and their acquisition of nitrogen was naught.

4. Under the same conditions the leguminous plants, after a period of nitrogen starvation, began to recuperate, the foliage returned to its normal green color, and the plants continued to grow, in some cases vigorously, to complete maturity, and acquired all the nitrogen necessary for this development.

5. The graminaceous plants are dependent upon the combined nitrogen of the soil for their development.

6. The legumes are independent of the combined nitrogen of the soil and can acquire all the nitrogen for their complete development from the air, and, furthermore, not from the small quantity of combined nitrogen contained in the air, but from the *free* nitrogen.

7. Whenever, under these conditions, the legumes acquired nitrogen, this acquisition was invariably accompanied with the appearance of tubercles on their roots.

8. Sterilization of the natural soil or of the soil infusion destroys its effect.

A year later, 1887, Wilfarth made a further report on this subject. In one experiment made by Hellriegel and Wilfarth the classical method of Boussingault was employed. They placed into a large glass globe four kilos of ignited sand, mixed with sufficient water and the necessary mineral constituents of plant food free from nitrogen compounds. They also added a small quantity, an aqueous infusion, of a soil in which peas had been previously grown. In the artificial soil thus prepared they planted a pea, a grain of oats and a buckwheat seed. The globe was hermetically sealed with a ground-glass stopper and the necessary carbon dioxide for the growth of the plants was introduced from time to time. The oat and buckwheat plants grew only till the seeds had become exhausted, and acquired no nitrogen in excess of that contained in the seeds. On the other hand, the pea plant made a vigorous and normal growth and was still growing, when the report was made. A large part of this plant had been removed and was found to contain 6.55 grams of dry matter and 0.137 gram of nitrogen.

This interesting experiment not only corroborates the claims of these investigators, but it completes the original experiment of Boussingault, in that it restores the condition of natural soils, which he had de-

stroyed by his method of removing fixed nitrogen. In this connection it is of interest to refer again to the position on the nitrogen question occupied alone by Ville. It can readily be understood that, in the large apparatus employed by this investigator, the chances for complete sterilization were very remote, especially since no particular attention was paid to this point. Microbes from the soil could easily have found their way into his large case through dust or otherwise, and in the presence of organic matter arising from the seeds and the roots of the plants, could, in a short time, become active in fixing the free nitrogen of the air. The contention of Ville that, in his experiments, free nitrogen of the air was assimilated by plants may, therefore, have been sound.

But to return to the line of thought broken by this digression, Wilfarth reported some important gains in nitrogen by lupines grown in pots with four kilos of nitrogen-free sand on addition of a measured quantity of soil infusion containing not more than seven tenths of a milligram of fixed nitrogen. The yields are as follows:

WITH SOIL INFUSION:

No. 3. 44.73 grms. dry matter with 1.099 grms. nitrogen.

No. 4. 45.62 grms. dry matter with 1.156 grms. nitrogen.

No. 5. 44.48 grms. dry matter with 1.194 grms. nitrogen.

No. 6. 42.45 grms. dry matter with 1.337 grms. nitrogen.

WITHOUT SOIL INFUSION:

No. 9. 0.918 grms. dry matter with 0.0146 grms. nitrogen.

No. 10. 0.800 grms. dry matter with 0.0136 grms. nitrogen.

No. 11. 0.921 grms. dry matter with 0.0132 grms. nitrogen.

No. 12. 1.021 grms. dry matter with 0.0133 grms. nitrogen.

By the sole employment of a small quantity of soil infusion containing an infinitesimal amount of combined nitrogen, in pots holding about eight pounds of sand, the plants made an average gain in dry matter of 42.9 grams, and in nitrogen of 1.18 grams over the same kind of plants grown under the same conditions without this addition. This remarkable result was surely worthy of the general interest which its publication evoked.

Numerous experimenters all over the world at once began to pay attention to the little tubercles, and they were investigated from all points of view. Their morphology was studied by Frank, Laurent and others. For this purpose Frank, as well as Laurent, grew plants partly in water culture with the production of root tubercles. Since their labors belong to the domain of biology this simple reference to them here will suffice.

The results of all investigations from a chemical standpoint verified the conclusions reached by Hellriegel and Wilfarth. But, in addition to this, a great many new facts bearing upon this subject were obtained. Bréal analyzed the nodules of various legumes and found that the content of nitrogen in the dry matter varied from three to seven per cent., and was higher than that of any other part of the plants excepting the seeds. This fact is significant.

Bréal also obtained results similar to those of Hellriegel and Wilfarth by germinating peas between moistened filter papers, inoculating the roots, after they had attained the length of a few centimeters, with a needle which had been plunged into a tubercle, and then growing the plants in nitrogen-free sand containing the necessary mineral ingredients of plant food.

This investigator also grew peas in water culture. After germinating seeds between moistened filter papers as before,

and after the roots had attained a length of three or four centimeters he inoculated them with a needle which had been inserted into a tubercle of alfalfa, and placed two of the young plants in a culture jar, which contained a nutrient solution free from combined nitrogen. The peas grew regularly so long as they found nourishment in the cotyledons. Then a period of nitrogen starvation set in, after which the plants recuperated and grew to maturity with the production of fruit. The period of vegetation extended from April 2 to June 10. At the latter date the roots contained numerous tubercles. The stalks and roots were separated, dried at 110° C. and weighed. The nitrogen of both portions was determined, as was also the weight and nitrogen of two seeds similar to those used in the culture experiments. The following table gives the results:

	Dry Matter, Grams.	Nitrogen, Per Cent.	Nitrogen, Total.
Stalks	3.785	2 35	0.089
Roots	1.165	2.60	0.030
Total	4.95		0.119
Seeds	0.502	3 60	0.018
Gain	4.448		0.101

The table shows that the plants contained ten times as much organic matter and six and six tenths times as much nitrogen as the seeds from which they were derived; also that the percentage of nitrogen of the roots was greater than that of the aerial portion. Now when it is considered that, in this experiment, there was no nitrogen compound of any kind present, except the infinitesimal quantity introduced by puncturing the roots with the needle, and that in two small plants there was a gain of 101 milligrams of combined nitrogen, the claim for the assimilation of free nitrogen must be regarded as established.

The order of leguminous plants, therefore, occupies a unique position in the art of agriculture. The experimental evidence herein submitted shows conclusively why leguminous crops have for ages been recognized as being of special value in maintaining soil fertility, and the discussion of this subject points to the fact that, in many walks and practices of life, empiricism has been in advance of science.

HENRY A. WEBER.
OHIO STATE UNIVERSITY.

SCIENTIFIC BOOKS.

Glacial Formations and Drainage Features of the Erie and Ohio Basins. By FRANK LEVERETT. U. S. Geol. Survey, Monograph XLI. Washington. 1902. Pp. 802; 26 pl. (maps, sections and views from photographs), and 8 figures in the text. $1.75.

Ohio is the central area described in this report, and it also includes parts of each of the adjoining states and of the Canadian province of Ontario. The great importance and interest of the glacial history of this region, early studied by Whittlesey, Newberry, Orton, Gilbert and N. H. Winchell, and later by Spencer, I. C. White, Wright, Claypole, Chamberlin, F. B. Taylor and many others, is indicated by about five hundred papers cited in a bibliography of twenty pages.

Mr. Leverett enumerates eleven epochs or stages of the glacial period, as follows: (1) The oldest recognized glaciation, called the sub-Aftonian by Chamberlin, perhaps the same as the Albertan of Dawson; (2) the Aftonian interval of recession of the ice sheet; (3) the Kansan stage of glacial readvance; (4) the second or Yarmouth interval of recession; (5) the Illinoian readvance; (6) the third or Sangamon recession; (7) the Iowan readvance, with the principal time of deposition of the loess; (8) the fourth or Peorian recession; (9) the early Wisconsin stage of readvance, with the formation of four successive systems of marginal moraines during the early part of the ensuing recession; (10) the fifth interval of glacial retreat,

with important changes in the outlines and relations of the ice lobes; and (11) the late Wisconsin stage of mainly continued retreat, with ten substages of halt or slight readvance, marked by a series of that number of marginal moraines and changes of the glacial lakes that finally occupied the Erie and Ontario basins. The chief part of the region is covered by the Late Wisconsin drift and its moraines, which in eastern Ohio extend to the boundary of the glacial drift.

Chapter III., filling a sixth part of the volume, treats of the drainage systems, noting in much detail the evidences of great modifications of the preglacial water courses. It is shown that before the ice age probably the upper and middle parts of the present Allegheny River were separately tributary to the stream then flowing along the present bed of Lake Erie; that the lower Allegheny and the Monongahela, with the upper Ohio River in Pennsylvania, flowed also north to the old River Erie by the valley of the Grand River; and that many other changes from the ancient courses of drainage also took place during the glacial period along the Ohio River, thence down to Cincinnati, where the ice sheet at its stage of farthest advance reached across that valley into the edge of Kentucky.

Descriptions of the various drift formations, and especially of the moraines, occupy the greater part of this monograph, which is the second of a series giving the results of Mr. Leverett's extensive field work. The first was published three years ago, entitled 'The Illinois Glacial Lobe,' and he has another in preparation, to treat similarly of the glacial and lacustrine geology of Michigan. His elaborate studies of the ice age in this region of the great Laurentian lakes, abounding with very instructive records of the oscillations and wavering departure of the continental ice sheet, and comprising at last a complex history of many small and large ice-dammed lakes, should be of much value as a basis of text-books for the schools and colleges of these states.

As soon as the recession of the ice sheet caused it to be a barrier on the northeastwardly sloping Erie basin, the water impounded there spread out as a lake, with outlet past Fort Wayne to the Wabash River. Its earliest stage is named Lake Maumee; a later stage, when a lower outlet was uncovered by the glacial retreat, past Ubly, in Michigan, is called Lake Whittlesey; and the still later and most extended stage of this body of water, reaching then into the Huron basin and outflowing, as Lake Whittlesey had done, to Lake Chicago in the southern part of the basin of Lake Michigan, retains the name Lake Warren, which was proposed by Spencer. The shores of these glacial lakes, marked by beach ridges of gravel and sand, have been traced from Fort Wayne east through Ohio, along the Erie shore of Pennsylvania, and to the Finger Lakes and beyond in central New York, where Fairchild has identified the routes of later eastern discharge by which Lake Warren was finally drawn away to the Mohawk and Hudson, being succeeded by the glacial lakes Algonquin and Iroquois in the Huron and Ontario basins.

While the ice sheet was melting away, the land on which it had lain was uplifted from a depression, so that the shore lines of the glacial lakes now have, along great portions of their extent, an ascent to the north and northeast, varying from a few inches per mile to a foot or more, and in some districts, notably east of Lake Ontario, even as much as five feet per mile. At the end of the Iowan stage of glacial advance, the deposition of loess in the Missouri and Mississippi valleys, and of a closely analogous silt formation in the Ohio valley, as described in this report, gives evidence of a depression of these regions probably several hundred feet below their present height. Before the accumulation of the moraines in the Wisconsin stages of general glacial recession, the greater part of the Mississippi and Ohio basins, and the southern part of the basins of lakes Michigan and Erie, had been reelevated to nearly the same altitude that they have since maintained with only slight changes. But after the moraines were formed, and during the existence of the great glacial lakes on the northern borders of the United States, much of their areas yet remained depressed, as is known by the in-

clination of the originally level shores of these lakes.

The latest completed geologic period, when an ice sheet covered the northern half of our continent, is being very satisfactorily investigated, both in the United States and Canada. As in an earlier monograph of this series, on the glacial Lake Agassiz, it will be an advantage to the geological surveys of each country that these detailed explorations about the Great Lakes be extended to give such full description and discussion of the ancient larger lake areas, with their shore lines and relations to the waning ice sheet, on both sides of the international boundary.

WARREN UPHAM.

SCIENTIFIC JOURNALS AND ARTICLES.

Bird Lore for November–December contains articles 'On Journal Keeping,' by Ernest Thompson Seton; 'Flamingoes' Nests,' illustrated, by Frank M. Chapman; 'The Weapons of Birds,' by F. A. Lucas; and 'Whiskey John in Colorado,' by E. R. Warren. The seventh paper on 'How to Name the Birds' is devoted to the Sylviidæ and Turdidæ and the first paper on 'How to Study Birds' are both by Frank M. Chapman. There is the first of a series of portraits of *Bird Lore's* advisory councilors depicting Messrs. William Dutcher, T. Gilbert Pearson, Lynds Jones and C. W. Nelson, and the usual notes, reviews and reports of societies.

The Museums Journal of Great Britain for November has an article on museum matters presented at the Belfast meeting of the British Association, and description of a dust-proof air inlet for museum cases, a feature entirely too much neglected in the construction of cases. F. A. Bather discusses 'Names on the Labels in Public Galleries,' in which he touches on the difficulties of providing so-called common names for objects and intimates that scientific names are much more generally understood than is often supposed. This article should be widely read. There is an interesting series of notes concerning museums in various parts of the world.

THE *American Museum Journal* for December gives a summarized account of the proceedings of the Thirteenth International Congress of Americanists, a review of the recent work of the museum, and a list of the December lectures. The number contains the index for Volume II.

The Plant World for October contains 'Extracts from the Note Book of a Naturalist on the Island of Guam,' by W. E. Safford; 'A Study of the Island Flora of the Mississippi River near Sabula, Iowa,' by T. J. and M. F. L. Fitzpatrick, and the second article on the 'Origin of Plant Names,' by Grace S. Niles. Among the shorter articles are the official announcements of the Wild Flower Preservation Society.

SOCIETIES AND ACADEMIES.

PHILOSOPHICAL SOCIETY OF WASHINGTON.

THE 558th regular meeting was held November 22, 1902.

Dr. H. Carrington Bolton presented a paper on 'Science and Art under Rudolph II., 1570–1612,' narrating many of his experiences with the astrologers and charlatans that he patronized so liberally, and pointing out the important results that followed his support of Tycho Brahe and Kepler.

Dr. A. F. A. King read 'Further Remarks on Sunlight, Malaria and Scoto-therapy,' in which he reviewed his former paper (see SCIENCE, December 27, 1901, p. 1007), and in support of the blue fluorescence of quinine being its curative property, cited the facts that *esculin* and *fraxin* were also fluorescent and curative like quinine. The curative power of iodine was due to its producing the *violet* iodide of starch in the stomach.

Dr. King recommended blue- or violet-colored clothing for armies in malarious regions, and purple tents instead of the white canvas now used. He suggested several experiments in scoto-therapy—keeping some patients in the dark or in rooms with purple or indigo window glass, and exposing others, nude, to brilliant sunshine—which were inexpensive and easily accomplished, and which, he hoped, those having opportunities would try, in order

to test the power of sunlight in promoting sporulation of malarial parasites in the blood.

Dr. Peter Fireman then spoke on the 'Deduction of the Magnitude of Osmotic Pressure according to the Kinetic Theory.' He held, first, that the mean kinetic energy of the molecules of a dissolved substance is the same as that of a gas at the same temperature; and, second, that the number of impacts of the molecules of a dissolved substance per unit of time on unit area of any imaginary plane in the solution is the same as if the dissolved substance were in the gaseous state and confined in the same volume at the same temperature. Therefore, the laws governing osmotic pressure in solutions are identical with the laws of perfect gases, and follow directly from the kinetic theory.

THE 559th meeting was held December 6, 1902.

Announcement was made of the death of Mr. Henry Mitchell, a distinguished engineer, and of Mr. J. W. Osborne, a distinguished inventor in the art of photolithography, both members of the society.

Professor Newcomb gave a brief account of his visit to Christiania during the past summer to attend the convention of mathematicians held in commemoration of the one-hundredth anniversary of Abel's birth.

The first regular paper was by Dr. C. D. Walcott on 'The Development of the Carnegie Institution.' He pointed out how its location in Washington is a case of natural development, tracing the growth of scientific organization in the city from the early days when this society stood alone, through the times when societies were multiplied, then through the unifying period of the Joint Committee and the Academy of Sciences, out of which came the Washington Memorial Association; by this last-named body plans for research were formulated clearly enough to attract Mr. Carnegie's attention. His $10,000,000 endowment of the new institution is familiar to all. The trustees of this body appointed 16 advisory committees, including 46 members; their reports on projects submitted to them, filling over 200 printed pages, were presented confidentially to the trustees at their recent meeting, together with other reports; portions of these will be made public early next year. Statements were made regarding the principles adopted for making grants, both of exclusion and of inclusion; special emphasis is laid on the selection of the man who is to be responsible for any specific research, since vague or general appropriations are not favored.

Dr. H. W. Wiley, of the Department of Agriculture, in view of the popular interest in his diet investigations, discussed 'How to Test the Harmfulness of Food Preservatives,' if they are harmful, as alleged. He called attention to the enormous industrial importance of the subject, the difficulty of obtaining reliable data, and the danger of complications with foreign countries over our food exports. The older methods of preservation were: Desiccation, resulting in a tasteless product; sterilization by heat, often imperfect, and cold storage. The cold storage plants of the country are worth $100,000,000 and contain constantly food supplies of an equal value. Cheapest of all methods is the use of chemicals. The effect of these may be tested, chemically by artificial digestion, by experiments on the lower animals, or by experiments on man. Under an appropriation from Congress the speaker is beginning experiments on twelve volunteers, whose food supply and excreta will be fully analyzed to determine the effect, if any, of the usual preservatives. Various details of the direct and the control experiments were given.

CHARLES K. WEAD,
Secretary.

GEOLOGICAL SOCIETY OF WASHINGTON.

THE 134th meeting was held December 10, 1902. The following papers were presented:

'A Carboniferous Section in the Upper Copper River Valley, Alaska,' by W. C. Mendenhall.

Mr. Mendenhall presented some of the details of a section of 7,000 or 8,000 feet of Upper Carboniferous strata, measured during the past summer among the foothills of the Alaskan Range, in the northern part of the

Copper River basin. The paper closed with a summary of the known Alaskan occurrences of the Carboniferous.

'Occurrence of Paleozoic Rocks in the Southern Portion of the Great Basin Region,' by F. B. Weeks.

Mr. Weeks said in part, the Paleozoic sedimentary series in this region extends from the Pre-Cambrian to the Permian, or possibly the Trias. The granites and allied rocks of the Grand Canyon section, and of southeastern California extending into the Sierra Nevada, comprise the basement complex. The Pre-Cambrian consists of quartzites and schists of undetermined thickness. These are conformably overlain by the Cambrian strata of alternating beds of quartzite, shale and limestone, which attain a thickness of 10,000 feet or more. The Silurian is represented by two great masses of limestone with several hundred feet of quartzite between them. The series is very similar to that described by Hague at Eureka, and the important unconformity between the quartzite and the overlying limestone noted at Eureka also occurs in the Panamint and Grapevine ranges. The Devonian limestone is exposed in the ranges directly east of the Grapevine range, and also forms a considerable portion of the Desert range. The Carboniferous limestones are exposed in the Inyo and Darwin ranges and form a large part of the Charleston mountains. The section in the latter range consists of Lower Carboniferous limestones, red sandstones and shales and an Upper Carboniferous limestone. Above these are other limestones containing a Permian or possibly a Triassic fauna. The data relating to the Charleston range were obtained by R. B. Rowe, who was engaged in a study of the geology of this region for some months prior to his death in 1902. Between the Cambrian and the Ordovician in the Great Basin region there appears to be heavy faulting in some sections, and in others a thrusting of the Ordovician upon the underlying Cambrian beds. Prior to the Carboniferous there was an erosion interval and an overlapping of the Carboniferous upon the Devonian and probably the Silurian. King's conclusion that there are 40,000 feet of conformable Paleozoic strata in the Great Basin region has not been confirmed by recent studies. The structure of the Basin ranges is believed to be the result of crustal movements of uplift and subsidence accompanied by faulting, thrusting and erosion at different stages of Paleozoic time, orographic forms having been modified by erosion and subsequent earth movements during the long interval from the Permian to the present.

'The Horseheads Outlet of the Glacial Lakes of Central New York,' by M. L. Fuller.

Mr. Fuller described the nearly uniform maximum altitude attained by the crests of the drift deposits of the valleys both to the north and to the west of Horseheads, and classed them with morainal terraces rather than with true moraines. The uniform altitude, together with the presence at several points of notches cut to approximately the same level across the projecting rock points by streams flowing along the sides of the ice tongues occupying the lower portion of the valleys, was considered as evidence of a local body of standing water reaching to a height of about 100 feet above the present streams at Horseheads. The terraced character of the outlet at Horseheads was also described, and the opinion expressed that the broader terrace is an erosional and not a constructional (flood-plain) feature, and that it represents the outlet of Lake Newberry at its principal stage. The lower and smaller channel, which is narrower than many of the channels cut by the small streams now existing, is considered as marking the final stage of the outlet when part of the escaping waters of the lake were, as the ice retreated, beginning to escape at other outlets located, it seems most probable, at a point some distance to the east.

ALFRED H. BROOKS,
Secretary.

TORREY BOTANICAL CLUB.

AT a meeting of the club on November 11, 1902, the scientific program consisted of a paper by Dr. L. M. Underwood on 'The Gold and Silver Ferns.' Dr. Underwood said that characters based upon position and form of

sori and indusia have perhaps been emphasized too much in classification; in some species the indusium may be developed or may be wanting on the same plant. There is now a tendency to return to the recognition of the fibro-vascular system as an element in classifying ferns. Mainly free-veined ferns occur in Devonian and Carboniferous remains. Anastomosing veins seem to have developed later; and even now they form the predominant feature in but two of the ferns of our northern states, *Onoclea sensibilis* and *Woodwardia areolata*. The pinnate and flabellate types of venation are very distinct, but are connected in appearance by a modification of the last type with successive alternation of its dichotomy forming a prolonged axis. The ferns known as gold and silver ferns were included in 1811 in the genus *Gymnogramme*. Some twenty genera have since been segregated from it, some of them on insufficient grounds. Many garden hybrids and horticultural varieties have been developed. With the exception of a species in Madagascar, the group is confined to the tropics of America, where the species known as the silver fern is perhaps the most common fern known. The goldenback fern of California is perhaps most familiar to ordinary knowledge; its range is from Alaska to Lower California, but not eastward of the Sierras. In life it is of a bright golden yellow beneath (often replaced by silvery powder), a brilliant green above; in the dry season it coils up involutely, exposing only the under surface, which is covered by its peculiar golden waxy powder.

This and other ferns of the arid region prevent too great transpiration of water by developing waxy or resinous powders, or by layers of wool or of scales. A Mexican species, *Notholæna aurantiaca*, was exhibited, which combines two protections, powder and scales. The silver fern of our arid Southwest finally becomes almost chalky beneath; it becomes coiled almost into a ball in the dry season.

Discussion followed upon the true interpretation of the function of the waxy powder. Dr. C. C. Curtis deemed it to accomplish two purposes, that of plugging stomata and that of reflecting heat. Dr. Rusby recalled the suggestion made by Mr. Chas. F. Cox some years ago, to the effect that plant hairs carry on metabolism and aid nutrition.

Dr. Rusby also described the appearance and habitats of several species which he had been familiar with in Bolivia and in our own Southwest; in the Rockies, where *Notholæna* and *Cheilanthes* grow together from the same crevices of rock, they respond to rain with remarkable quickness. In the dry season when everything else is seemingly dead, if a rain should occur, their coiled fronds quickly become bright green and well expanded, though curled again into little balls in a few days if dry weather follows.

EDWARD S. BURGESS,
Secretary.

NORTH CAROLINA SECTION OF THE AMERICAN CHEMICAL SOCIETY.

THE North Carolina Section held its fall meeting in the Office of State Chemist, Agricultural Building, Raleigh, N. C., on Saturday, November 22, 1902, with presiding officer Charles E. Brewer in the chair. Twenty members and visitors were in attendance. Hereafter all papers presented at the meetings will be required to be in abstract. Drs. A. S. Wheeler and G. S. Fraps were elected reviewers for the Section for the ensuing year. Their duties will be for each to prepare and present at some meeting during the year a paper giving briefly the advances recently made in some branch of chemistry. This departure promises to be a valuable addition to the programs. The following papers were presented and discussed:

'Some New Double Sulphates of Lanthanum, and on the Existence of Lanthanum Alums,' by Charles Baskerville and E. G. Moss.

'Lanthanates,' by Chas. Baskerville and G. F. Catlett.

The resemblance of lanthanum to aluminum was taken advantage of and the preparation of such bodies as the lanthanates and meta-lanthanates hitherto not reported, described. The new substances are sodium lanthanate ($NaLaO_2$) and meta-lanthanates of sodium,

potassium, lithium and barium ($M'H_4La_5O_{11}$). Two methods were used—fusion of lanthanum oxide with alkaline carbonates, and prolonged digestion in a very concentrated solution of the alkaline hydroxides at 100° C.

'Studies in Nitrification,' by G. S. Fraps.

The nitrification of ammonium sulphate or cotton-seed meal in a soil under constant conditions is periodic, reaching a maximum and then decreasing, due probably to the variation in the activity of the nitrifying organisms at different times. A sterilized soil, inoculated with different nitrifying soils, nitrifies cotton-seed meal and ammonium sulphate in different ratios, according to the soil used for inoculation, due to difference in the nitrifying organisms. A method is given for comparing the nitrifying power of two or more soils.

'Improved Method for Halogen Determinations in Atomic Weight Work,' by Chas. Baskerville and R. O. E. Davis.

The method reported was devised in the progress of the work on the redetermination of the atomic weight of thorium. The numerous precautions for the determination of chlorine were rehearsed, and attention directed to the deliquescence of thorium tetrachloride and the difficulty incident to complete elimination of chlorine from the dioxidein obtaining the ratio between the halogen and oxygen compounds of that heavy metal. A series of twenty-five preliminary determinations was made of the solubility of silver chloride in pure alcohol, alcoholic solution of silver nitrate, and nitric acid of variable strengths at different temperatures with a time variant. All reagents were the so-called chemically pure.

Elevation of temperature (50° C. and above), excess silver nitrate (more than twenty per cent.), marked acidity (over three per cent.) and prolongation of time of reaction (fifteen minutes) were determined as factors causing a result too high by from .7 to 4.3 per cent. (in exaggerated cases) when a standard sodium-chloride solution was precipitated by a standard silver nitrate. This was due to the formation of aldehyde from oxidation of the alcohol by the nitric acid and silver nitrate, with consequent precipitation of metallic silver with the silver chloride. Experimental proof of this was given.

A new series of six determinations, where all reagents were repurified, silver nitrate being made from metal prepared by the method of Stas, was carried out. Results were obtained giving an error of from zero to .098 per cent., hence it appears that the halogen may be determined accurately when an excess of silver nitrate is used (even to ten per cent.) the solution is slightly acid (nitric), the precipitation being caused at ordinary temperatures with vigorous stirring for five minutes in ethyl alcohol. Proper precautions as to purification of asbestos, using counterpoise crucibles, dark chamber for precipitation and filtration, dark bath for drying, etc.

The use of alcohol appears to be new.

'Chlorides in Tobacco,' by W. H. Pegram. The work set forth in this paper was designed and is being prosecuted for the purpose of ascertaining whether there is a relation between the chlorides in tobacco and the chlorides in the fertilizer used in its production; also whether a high percentage of chlorides (as calcium and magnesium chlorides) affects the hygroscopic property of tobacco, giving abnormal and damaging results at certain stages of its preparation and manufacture. The data are insufficient as yet to justify a valid conclusion.

'Suggested Changes in the Law of Dulong and Petit,' by J. E. Mills. Abstract has appeared in the proceedings of the Elisha Mitchell Scientific Society. (See SCIENCE, N. S. Vol. XVI., No. 414, p. 907.)

'Neodymates,' by Charles Baskerville and W. O. Heard.

The following methods were used in efforts to prepare neodymates: Fusion with alkaline carbonates, alkaline earth carbonates and oxides, chlorides, digestion in concentrated alkaline hydroxide solutions, and fusion with sodium dioxide. Results not altogether satisfactory were obtained, with the surprising exception of a barium compound,

$$BaCO_3.Ba(NdO_2)_2.5H_2O.$$

'Artificial Plant Food Requirements of Soils,' by B. W. Kilgore. (See 'Proceedings of the Fifteenth Annual Convention of the Association of American Colleges and Experiment Stations,' pp. 73–75.)

'Methods for the Determination of Total Phosphoric Acid and Potash in Solids,' by C. B. Williams.

The method devised for the determination of total phosphoric acid in soils was simply, after igniting five grams of soil in a platinum dish, treat three times with hydrofluoric acid, evaporating to dryness each time, followed by fusion with ten grams of a mixture of equal parts of sodium and potassium carbonate. The cake thus obtained, after cooling, was transferred to a beaker and digested with about 30 to 40 c.c. (1 to 1) hydrochloric acid, after which the solution was evaporated to dryness on a water-bath, being subsequently heated four or five hours in an air-bath, to 110° C. to dehydrate the silica present. It was then taken up with dilute hydrochloric acid, filtered and washed. The filtrate and washings thus obtained, after adding sufficient nitric acid to liberate all hydrochloric acid present, are placed together and reduced to a volume of about 40 c.c. by boiling. The excess of nitric acid is then neutralized with ammonia, and ten to twelve grams ammonium nitrate is added. After cooling, 30 c.c. recently filtered molybdic solution is added and the phosphoric acid is precipitated by shaking in a Wagner machine, and determined volumetrically (*Jour. Am. Chem. Sc.*, Vol. 23, No. 1, pp. 8–12).

Total potash is brought into solution by treating four grams of soil in a platinum dish on water-bath, after saturating with dilute (1 to 1) sulphuric acid and igniting, with from 2 to 3 c.c. hydrofluoric acid for five times, adding 1 c.c. dilute (1 to 1) sulphuric acid just before going to dryness the last time. After the last traces of hydrofluoric acid have been liberated the dish is removed from water-bath and heated gently over small flame until evolution of sulphur trioxide ceases. The soil is then taken up with 20 c.c. distilled water slightly acidified with hydrochloric acid, and digested on water-bath until the solution has been reduced to about one third of its orginal volume, after which it is transferred to a 200-c.c. graduated flask and heated on water-bath to near boiling, when ammonia and ammonium oxalate are added in sufficient quantity to precipitate all iron, alumina and lime present. After cooling, the volume is made up to 200 c.c., and an aliquot corresponding to two grams is filtered off into a porcelain dish. From this point on the procedure is the same as that prescribed in the regular Lindo-Gladding method.

There being no further business, the section adjourned, subject to the call of the Executive Committee.

C. B. WILLIAMS.
Secretary.

DISCUSSION AND CORRESPONDENCE.

PRESIDENT SCHURMAN ON THE EDUCATIONAL REQUIREMENTS FOR PROFESSIONAL STUDY.

TO THE EDITOR OF SCIENCE: In the issue of SCIENCE of November 21, on page 816, is published an excerpt from the annual report of President Schurman of Cornell University, containing statements bearing upon the question of collegiate work as a requirement for admission to professional schools. It is not my function to discuss or criticize the policy of the President of Cornell University. The report, however, contains several statements upon which comment seems necessary.

"At Cornell University at any rate [runs the report] the established policy is to admit students to any course who are able to pass the examinations qualifying them to pursue that course. And such preliminary tests, it is generally conceded by the members of the profession concerned, do not exceed the requirements for graduation at the best high schools."

I cannot speak for the lawyer, the engineer or the architect, but in the name of the profession of medicine I beg in the most respectful manner to protest. With the matter of culture-studies we need have no concern here. I believe it may be stated as an established fact that a proper education in modern

medicine can not be acquired upon the basis of a high-school preparation. For the adequate study of modern medicine collegiate training in physics, chemistry and biology is essential; to use an academic term, they are prerequisites. How much collegiate training in these branches is necessary is open to discussion; the most general opinion among teachers of medicine is that two years are sufficient. I beg to state that, in my opinion, the majority of the members of the medical profession of this country, as represented in the recognized societies, do not believe that the tests preliminary to medical education need not exceed the requirements for graduation at the best high schools. The teachers of medicine may be said to be almost unanimous in the belief that collegiate preparation in the sciences is necessary for the study of medicine. The majority of the high-grade medical schools have already either inaugurated or announced collegiate requirements for admission; with other institutions the maintenance of the older system is purely a matter of present financial conditions and does not reflect the real policy. These changes have not been made in spite of the profession, but rather with the sympathy and support of the best elements in the profession. In any event, ought it not to be the function of the universities to lead and not to follow the professions?

There is a quite current confusion of two movements. One is the culture requirement for entrance upon professional study; the other is the training requirement. Knowledge of Greek literature, of esthetics, of political science, may be advantageous to the physician, but it is not essential to the study of medicine; knowledge of and training in physics, chemistry and biology of the collegiate type and quality are necessary for the proper study of medicine as it is taught to-day in our best institutions. Departments of medicine are requiring collegiate preparation; in a few instances it may be partly upon the basis of a veneration for general culture, in all instances from the realization of the direct necessity of that training in the natural sciences which colleges alone are able to give. With this adjustment of the prerequisites in science deemed necessary to the study of medicine, the matter of democracy in educational policy, alluded to by both President Schurman and President Hadley, has no concern. The science of medicine has developed to such an extent that it can not be so mastered in four years following a high-school education as to adequately prepare the physician for his duties in life. To extend this course, by prerequisite collegiate work, until it fulfills its obligations to the student and its duty to the public, can not be stigmatized as undemocratic.

ALONZO ENGLEBERT TAYLOR.
THE UNIVERSITY OF CALIFORNIA,
December 1, 1902.

ILLEX ILLECEBROSUS (LESUEUR), THE 'SQUID FROM ONONDAGA LAKE, N. Y.'

THE specimen of squid, the capture of which in Onondaga Lake has been described by Dr. John M. Clarke in a previous number of SCIENCE,* has been sent to the present writer for examination. It proves to belong to the well-known species of our northern Atlantic coast, the 'cold water' or 'short-finned squid.' The specimen has been compared with the description of *Ommastrephes illecebrosus* given by Verrill,† and with two well-preserved individuals (male and female) of this species from Provincetown, Mass., preserved in the collections of the Museum of Biology, J. C. Green School of Science, Princeton University. The result of this comparison is as follows:

Total length of our specimen, from tip of tail to tip of third pair of sessile arms, upward of thirteen inches. Since the largest figure for this dimension given by Verrill is a little over fourteen inches, our individual

* December 12, 1902, p. 947.

† *Ommastrephes illecebrosus* (Lesueur): Verrill, A. E., 'North American Cephalopods,' in *Trans. Connect. Acad.*, Vol. 5, 1880, p. 268, pl. 28. According to the 'Synopsis of Recent Cephalopoda' given by Hoyle, W. E., in 'Voy. *Challenger* Zool.,' Vol. 16, 1886, p. 34, the name of this species stands now as *Illex illecebrosus* (Les.).

is to be regarded as full grown. Other dimensions can not be taken on account of the distorted condition of the respective parts.

Body and head somewhat contorted and out of shape. Skin largely mutilated and worn off. General form agreeing with that of this species. The same is true of the shape and size of the caudal fin, which exhibits the characteristic outline. Details of head normal. Opening of the lids of the eyes widely distended, irregularly circular, anterior sinus indistinct (this is apparently due to preservation).

Sessile arms agreeing in size and shape with this species. All marginal membranes (outside of the suckers) very slightly developed (or worn off), the dorsal and lateral folds of these arms indistinct, and this is especially true of the high median keel of the third arm, of which only traces are seen in our specimen. Owing to the slight development of these keels all the arms appear less angular and more rounded in cross section, although the typical shape is still indicated. Tentacular arms agreeing completely with this species, only the keel on the back side of the club is less strongly developed. Marginal membranes of the suckers indistinct.

No hectocotylization on the fourth sessile arms visible; thus our specimen seems to be a female.

Arrangement, size and structure of suckers of the sessile as well as the tentacular arms agreeing perfectly with Verrill's description and the specimens used for comparison; the only difference I see is that outside of the two rows of large suckers of the club of the tentacles there are only a few smaller ones; but these may in part have been torn off and lost.

The buccal membrane agrees with this species. Color, yellowish-white, with purple chromatophores, but skin largely damaged, so that the usual color pattern is not visible; but the dark blotches above the eyes are well marked. The pen has not been taken out.

To sum it up, our specimen agrees in all essentials with *Illex illecebrosus;* the only differences observed, namely, the wide eye opening, the lack or slight development of the marginal membranes and the keels of the arms, and the absence of some suckers on the tentacles, are no doubt due to preservation and rough handling. That the latter has taken place is shown by the general abrasion of the skin, and the fact that a large number of the suckers have lost their horn rings or are entirely torn off. Similar mutilations and changes are very often observed in ill-preserved cephalopods. Therefore, I arrive at the conclusion that the present individual is in no wise different from *Illex illecebrosus* of our northeastern coasts.*

As to the alleged capture of this species in Onondaga Lake, I can only refer to what Dr. J. M. Clarke says (*l. c.*), and if it is a fact that this species lives in this lake, the only explanation is, as suggested, by a former, post-glacial connection of this lake with the St. Lawrence Gulf. But I am loath to believe that this species *lives* in Onondaga Lake. In this connection I venture only one single suggestion: this squid is largely used for bait, and the capture of squid forms a regular trade on our northeastern coasts. Could it not be possible that somebody has secured by purchase a barrel of squids, to be used as bait at the locality where our specimen was found?

A. E. ORTMANN.

PRINCETON UNIVERSITY,
December 12, 1902.

* This species is abundant from Cape Cod to Newfoundland. Rarely it is found to the south of this range (Vineyard Sound and coast of Rhode Island).

KALLIMA BUTTERFLIES.

TO THE EDITOR OF SCIENCE: Dr. Bashford Dean will find some interesting remarks on the mimicry by this butterfly, and some criticisms of museum representations of it, in an interesting article by E H A 'On the Influence of Mind in Evolution,' *Natural Science,* Vol. IX., pp. 297–302, November, 1896. The main point as regards museums made by this competent observer is that he never saw a *Kallima* sitting with its apparent stalk towards the twig of a tree. On the contrary, it 'always alights head downwards, so as to face anything coming up the tree, which is

much the most likely direction of assault from a lizard.' According to this writer, it is when the butterfly requires to rest that it settles, not on the under side of a leaf, as do most other butterflies, but 'on the bare trunk, or one of the larger boughs, of a tree.'

NAT. SCI.

SHORTER ARTICLES.

DATA WITH A POSSIBLE BEARING ON THE CAUSE OF LIGHTNING.

1. LENARD inferred from his experiments that it is necessary for the water jet to strike a solid obstacle to generate the electricty observed in the surrounding medium of air. I find that a surface of water is also efficient, and I place the electricity as a charge on the water nuclei produced, because the charge increases with the number of nuclei computed from their coronas. In other words, the mere attrition of water by water is sufficient to charge the nuclei.

In a forthcoming paper in the *American Journal of Science* I show that if each nucleus carries one electron, there must be at least 10^6 nuclei per cubic cm. 'At least,' because much of the charge is lost in the tube which conveys the nuclei into the condenser, and I have not yet allowed for this.

From the coronas simultaneously produced I find that about 5,000 nuclei are present per c.c. Hence each nucleus carries 200 electrons, while its potential is below five volts. Thus there are a million electrons in each c.c. of the air which I examine, or in a cubic kilometer there would be 10^{21} electrons, or 7×10^{11} electrostatic units of charge, or about 200 coulombs.

2. Let this region be spherical with a superficial capacity, which would then be $.62 \times 10^5$ cm. The potential* at the surface of the region would be eleven million electrostatic units of potential, or over three thousand million volts, if the nuclei were all of the same sign and were transferred to the surface. Every time the region is emptied of its nuclei, the surface acquires a charge of the enormous potential stated, and there is no reason why the nuclei may not be continually produced by attrition while they are being transferred.

3. Now regarding the transfer of nuclei, we may note that when they are produced from pure water, positive charges are usually in excess; when produced from dilute solutions, negative charges are usually in excess; but I find that the bulk of the nuclei are symmetrically positive and negative.

The velocity of the nucleus of charge e, in an electrostatic field of the potential gradient, E, is $v = Ee/6\pi\mu R$, where R is the radius of the nucleus and μ (.0002, say), the viscosity of air. Put, therefore, in this equation the values which I have here and in other places adduced, $R = 10^{-6}$ cm., $e = 200 \times 7 \times 10^{-10}$ electrostatic units, whence $v = 37$ cm./sec., or over four fifths mile an hour, for the unit electrostatic field; about .003 mile per hour of a field of one volt/cm.

Thus there is considerable mobility in these nuclei. With a strong electrostatic field at least locally in action, the nuclei of one sign would thus be driven outward, warmer nuclei into colder regions of continually increasing conduction or rarer air, where their charges would be dissipated. The other nuclei would be driven earthward, colder nuclei into warmer regions of continually decreasing conduction to be discharged, if at all, by a flash, particularly if, on growth of drops, gravity steps in as a final motor.

Whether there is sufficient commotion in thunder-storms to give rise to the attrition of water, whether comminution will not suffice if accomplished in other ways, whether an earth electrostatic field is an adequately permanent or localized occurrence, whether indeed separation of nuclei is needed if there is enough excess of charges of definite sign, must be left for further consideration; but it seems to me that data bearing on the occurrence of lightning are here suggested which deserve serious scrutiny.

C. BARUS.

BROWN UNIVERSITY,
PROVIDENCE, R. I.

* For a mile flash of lightning 70 coulombs at a million volts are usually conceded (Lodge).

THE HOSTS OF ARGULIDS AND THEIR NOMENCLATURE.

AN excellent monograph of the 'North American Parasitic Copepods of the Family Argulidæ' has been contributed to the Proceedings of the U. S. National Museum by Dr. Charles Branch Wilson and just published. As it is 'the first of a series, now in course of preparation, on the parasitic Copepods,' it seems advisable to point out a defect which should be avoided in the subsequent monographs. The hosts are very often erroneously named or named in a very archaic or contradictory manner. The archaic nomenclature is chiefly connected with foreign forms and is the result of determinations of species made many years ago.

The host of *Argulus nattereri* (p. 720) and *Dolops longicauda* (p. 732) named '*Salmo* (*Hydrocyon*) *brevidens* Cuvier' (p. 720) or '*Hydrocyon* (*Salmo*) *brevidens* Cuvier' (p. 732) does not belong to the same order as *Salmo* nor to the same genus as *Hydrocyon* (which is confined to Africa), but to a genus (*Salminus*) peculiar to South America. The *Argulus salmini* (p. 720) was also found parasitic 'in the gill cavity' of *Salminus* and not of '*Salmo*,' a genus, as already stated, of a different order.

Species of '*Chromis*' are designated as the hosts of two species of Argulids, *Argulus chromidis* of Nicaragua (p. 721) and *Chonopeltis inermis* of Wiedenhafen, East Africa (p. 729).

Probably the Central American fish is a Cichlid of the genus *Heros*, and the East African, one of the genus *Tilapia*. *Chromis* is now reserved by all the best authorities for a salt-water genus of the family of Pomacentrids.

The host of *Argulus doradis* called *Doras niger* (p. 734) is now known as *Oxydoras niger*. The host of *Argulus africanus* (p. 727) called *Claria* is a catfish of the genus *Clarias*.

The host of *Dolops reperta* of Guiana (734) called '*Aymara*' is an Erythrinid now known as *Macrodon tareira* or by the earlier but extremely inappropriate name *Macrodon malabaricus*, due to a blunder of Bloch committed more than a century ago.

The host of *Dolops striata* (p. 735) and *Dolops bidentata* (p. 736) of Guiana, called 'a species of *Anguilla*,' is probably a species of a different order named *Synbranchus marmoratus*. No *Anguilla* has been recorded from Guiana.

The host of *Dolops discoidalis* designated as a species of *Platystoma* has been for nearly forty years universally called *Sorubim*.

Another fish, the common alewife, on the same page is called *Clupea vernalis* and *Pomolobus pseudoharengus*.

Dr. Wilson's bibliography is well digested, but he seems to have overlooked a few articles. Among such are three of minor importance by Reinhardt (1864), Frauenfeld (1870) and Dambeck (1877), besides one of considerable importance by von Nettovich (1900) of thirty-two pages and two plates.

One other defect should be remedied. No habitat except 'Wiedenhafen' is given for *Chonopeltis inermis*. As Wiedenhafen is not noticed in current gazetteers (it is not in the latest edition of Lippincott's) it was deemed necessary to refer to the original description but the only reference to the place of description was 'Thiele, 1901,' the rest of the line sufficient for the page being left blank. On reference to Thiele's article in the *Zoölogischer Anzeiger*, it appeared that Wiedenhafen is in East Africa. The name of the host is no guide.

The other lapses are not of sufficient importance to demand special attention here.

THEO. GILL.

COSMOS CLUB.

THE GREAT NEED IN AMERICAN ZOOLOGY.

AT the present day the zoologists of the United States of America can point to a considerable number of well-equipped laboratories, and of others in course of construction; of libraries, such as that of the Philadelphia Academy of Natural Sciences, which is probably not excelled; of an annually increasing number of fellowships and free scholarships to enable students to investigate; and of the aid of the government in maintaining such

institutions as the National Museum. Universities are growing richer, for which we are thankful, and more numerous, an evil necessary perhaps to the geographical extent of the country. There are great reference museums in Philadelphia, Washington, Boston, New York, Chicago, and others with a good promise that have been more recently started. These are surely signs of a vigorous activity in research, and we all must rejoice in them.

It is not buildings, nor endowment funds, nor libraries nor collections that make laboratories or universities or museums, but it is the men who do constructive work in them, those who discover and classify the facts. There have been examples of institutions that might have been splendid, but which have proved to be only ornate, and because capable men have not been placed in untrameled guidance of them they have proved to be melancholy mausoleums, examples of a donor's folly. They have had their use in the general economy of things, however, for they have taught the American public that men, and not buildings, mean greatness—the men who do the work for the love of it and without thought of personal advancement.

But the work that is being accomplished, the zoological investigations and reflections, what is being done to give it publicity? By no means all that should be done. The avenues of publication are incommensurate with the amount of the investigations. For we see nearly annually papers by Americans published in the English *Quarterly Journal of Microscopical Science,* in Spengel's *Zoologische Jahrbücher,* in the *Archiv für Entwicklungsmechanik,* and in the two *Anzeigers.* America builds and maintains laboratories in sufficiency, but does not afford to publish all the work done in them. One hesitates to undertake an elaborate contribution, particularly one with expensive illustrations, for when an American journal has at last been persuaded to accept it, great delay is experienced before its final appearance, and by the time the proofs are received they seem like an old and stale story. So we are obliged to advise our students to condense their doctor's theses, to omit colored drawings, even to use the pen in place of the pencil, in order to avoid the expense of lithography. Now any one at all conversant with the nature of zoological investigation understands how important for the representation of the facts are good and numerous figures; so important, that the zoologist is involuntarily inclined to estimate the truth of the facts contained in a paper by the character of the drawings, these being the concise evidence of what the describer has seen, or of what he thinks he has seen. The number of illustrations should in no case be reduced; in most cases they should be considerably increased, and as far as the mere statement of facts is concerned the illustrations should preponderate over the text. More thought goes into the making of a drawing than into the writing of a purely descriptive text, and much more technique. There would be much less confusion in descriptions, consequently much less also in conclusions, if writers had not been obliged to be sparing with their drawings, but every American editor shrinks before an offering of drawings. A certain German cytologist, as it will be recalled, sent out with each author's reprint of a paper upon cellular 'Elementarorganismen' a small ribbon of paraffine sections of the objects that he described, with the request that each recipient mount these sections, study them, and so be convinced of the writer's truth. That is a method of argument, however, that is generally not feasible; duplicate material cannot be furnished to all who are interested in a subject, but good drawings and plenty of them should be furnished, regardless of the expense of reproduction.

Plainly, what we need, and it is now the first need of zoological research, are ample means for publishing large monographs accompanied by numerous detailed plates, and for publication of them as rapidly as the plates can be reproduced. Our present journals are mainly the proceedings, transactions and memoirs of societies and universities, and the government publications; there are a considerable number of these, and some of them offer excellent facilities. Then there are a

few independent journals for general zoological papers, such as the *American Naturalist* and the *Biological Bulletin*, both intended for shorter contributions; and the more recent *Journal of Anatomy*, which is limited, however, mainly to vertebrate anatomy. Foremost among the independent journals is the *Journal of Morphology*. It has done its duty nobly; we are proud of it and ready to maintain it; but it should have two or three volumes a year, instead of a single one, and as many more as may be necessary.

That these avenues of publication are far from sufficient for the amount of investigation is shown by the fact, already mentioned, that a large number of American papers are being published abroad, and that American editors are obliged to insist upon small volume of text and paucity of illustrations. Occasionally a Mæcenas has come forward and made possible the publication of a large work; but obviously investigation cannot depend upon such sporadic aid. Contrast our relatively small number of journals with those in Germany. There, in addition to the publications of societies, which are more numerous than our own, and some of them much more sumptuous, are a large number of independent journals: the *Anatomischer Anzeiger*, *Zoologischer Anzeiger*, *Biologisches Centralblatt*, and others intended for shorter papers; and for larger monographs the *Zeitschrift für wissenschaftliche Zoologie*, *Archiv für mikroskopische Anatomie*, *Morphologisches Jahrbuch*, *Jena'ische Zeitschrift*, *Zoologische Jahrbücher*, *Anatomische Hefte*, *Ergebnisse der Anatomie und Entwicklungsgeschichte*, *Archiv für Naturgeschichte*, *Archiv für Protistenkunde*, and others. America can make absolutely no comparison with that array, which includes only the more notable journals. France and Austria also outdo us in facilities for publication.

To our shame it must be said that our avenues of publication by no means keep pace with the increase in work of investigation. It is not a new fact; it is a case of bringing owls to Athens to recall this state of affairs to the readers of SCIENCE. But the condition of apathy that has existed in regard to it needs to be replaced by one of activity. There are rich men who can financier our zoological publications if the matter be brought to their attention in the right way; an ample endowment fund for large monographs, safeguarded by a competent board of critical editors, is not chimerical, but entirely feasible. The society should feel itself honored by the tender of a good monograph, and not the author by its acceptance for publication; good work should not go a-begging. There should be a concerted attempt to strengthen all the present journals, by increasing already existing publication funds and by multiplying the number of subscribers. Can not the matter be so presented to rich men that they may see an endowment fund for publication is of greater service than the founding of a university? Few men are so made that they have so much delight in the discovery itself, that the charm is not enhanced by making it known to others; obstacles in the way of publication, such as there are to-day without need, may do much to dishearten research.

One word of warning must be said: we do not need new journals, but a financial strengthening of those that we already have. And because, first, we owe support to the journals that have stood by us; second, because concentration is wiser than extensification, and, third, because a new journal, whose name has not yet become known, means practical burial for the papers contained in its earlier issues.

THOS. H. MONTGOMERY, JR.
UNIVERSITY OF PENNSYLVANIA.

THE BISHOP COLLECTION OF JADE AND HARD-STONE OBJECTS.

HEBER R. BISHOP was born March 2, 1840, at Medford, Mass., and died in New York City, December 10, 1902, at the age of sixty-three years. Mr. Bishop recently presented his famous collection of jade and hard-stone objects to the Metropolitan Museum of Art, New York City, and gave the sum of $55,000 for its installation in suitable cases, to be made in Louis XV. style by Allard, of Paris, one of the leading artisans of France.

And if this is not enough his estate will add to this.

A special hall will be set aside for it, to be known as 'Bishop Hall,' where it will be displayed in the finest solid gilt-bronze, plate-glass cases, but it will not be upon exhibition until a year from this time.

This is the finest collection of jade objects, engraved and jeweled, that exists in any public museum or private collection. It numbers more than one thousand specimens and fully represents all phases of the artistic development of this interesting material. The collection was started by the purchase of the Hurd jade vase from Messrs. Tiffany & Co., in 1878. This was one of the finest pieces that ever left China, and led to Mr. Bishop's taste for collecting such objects.

The collection will be described in a volume, which when published will probably be one of the most remarkable, expensive and sumptuous books ever issued, limited to an edition of one hundred copies.

Nearly ten years ago, Mr. George F. Kunz began the preparation of a mineralogical, geological and archeological description of the collection, to be published in this great catalogue, upon which Mr. Bishop had expended more than $100,000 at the time of his death. The scientific investigation was given entirely to Mr. Kunz, and he associated with him about twenty of the most eminent men in various related lines upon both sides of the water; and a more thorough investigation of this mineral has been made than was ever perhaps undertaken upon any other known mineral. The specific gravity, the tensile strength, the compression test, the sonorousness of the mineral from a musical point of view; a chemical investigation, a microscopical study, a microscopical examination of the thin sections; the origin of the mineral, the mining, the archeological history; the cutting, drilling, polishing, and many other phases, were gone into most thoroughly; and where a specialist existed who more minutely understood any special branch, he was called upon by Mr. Kunz to carry out the work.

The volume upon publication will go only to public institutions. The foreign etchings by French and Chinese colonists are unequalled. Many of the color illustrations are by Prang, who made those in 'Gems and Precious Stones of North America,' so well known by our readers. It was this work that suggested the color illustrations for the Bishop book on Jade, as well as for the magnificent Walters book on Chinese porcelains.

SCIENTIFIC NOTES AND NEWS.

THE Nobel Prizes, if the statement now cabled from Sweden is correct, have been awarded as follows: *Medicine,* Major Ronald Ross, of the School of Tropical Medicine, Liverpool. *Chemistry,* Professor Emil Fischer, of Berlin. *Physics,* divided between Professors Lorenz and Zeemann, of Holland.

THE Cambridge Philosophical Society has elected as honorary members Professor H. F. Osborn, Bayley Balfour, A. H. Becquerel, E. Fischer, Richard Heymons, J. H. van't Hoff, M. Jordan, W. K. von Röntgen, Corrado Segre and Hugo de Vries.

MR. PHILIP MACMILLEN, director of the Queensland Botanic Garden at Brisbane, has been elected a corresponding member of the Royal Botanic Society of London.

W. H. OSGOOD, of the U. S. Biological Survey, has just returned from a biological exploration of the base of the Alaska Peninsula and the region between Lake Clark and Nushagak River. This work is in continuation of his previous explorations of the Yukon River and Cook Inlet regions, the results of which have been already published in North American fauna.

PROFESSOR J. C. ARTHUR has been granted a month's leave of absence by the authorities of Purdue University, and will spend January at the N. Y. Botanical Garden in researches on the genera of the Uredineæ and their types.

DR. M. A. HOWE, assistant curator of the N. Y. Botanical Garden, has returned from a six week's collecting trip along the coast of Florida, bringing a large number of specimens of the algal flora of the Keys. Professor

F. S. Earle, assistant curator, returned from Jamaica on December 2. During his tour on the island of Jamaica an investigation was made of a number of diseases of the economic plants and a large collection of fungi was made.

MR. HANBURY, fellow of the Royal Geographical Society, reached Winnipeg on December 15, after an absence of nearly two years in the Arctic circle and the Hudson's Bay regions.

As has been fully reported in the daily papers, Mr. Marconi has established communication by wireless telegraphy between Cape Breton and Cornwall. His announcement, dated December 21, is as follows: "I beg to inform you for circulation that I have established wireless telegraph communication between Cape Breton, Canada, and Cornwall, England, with complete success. Inauguratory messages, including one from the Governor General of Canada to King Edward VII., have already been transmitted and forwarded to the Kings of England and Italy. A message to the *London Times* has also been transmitted in the presence of its special correspondent, Dr. Parkin, M.P."

DR. W. B. WHERRY, associate in bacteriology at the University of Chicago, has received an appointment to the post of bacteriologist in the Government Laboratories at Manila, P. I.

DR. DAVID T. DAY, chief of the Division of Mineral Resources of the U. S. Geological Survey, has been elected a member of the board of managers of the National Geographic Society to fill the unexpired term of Mr. Henry Gannett. As Mr. Gannett will remain in the Philippines for a year or more, engaged in the census of the islands, he has resigned temporarily from the board.

THE first Livingstone gold medal has been awarded by the council of the Scottish Geographical Society to Sir Harry H. Johnston, G.C.M.G., K.C.B., for his distinguished services as an explorer and administrator in Africa.

MR. EDMUND PERRIER has been elected as the representative of the Paris Museum of Natural History on the French Council of Public Instruction.

DR. J. B. DETONI has been appointed professor of botany and director of the Botanic Gardens at the University of Modena.

PROFESSOR G. W. GREEN, professor of mathematics in the Illinois Wesleyan University, has died at Bloomington, Ill., at the age of forty-five years.

WE learn from the *American Geologist* of the death of Mr. R. A. Blair, at Sedalia, Mo. He had spent many years in studying the rocks of central Missouri, and had made valuable collections from the Chouteau limestone.

THE death is announced of Dr. J. Wislicenus, professor of chemistry in the University at Leipzig.

PRIVY COUNCILLOR VON KUPFFER, professor of anatomy at Munich, died on December 16.

WE regret also to record the deaths of Dr. Friedrich Rüdorff, formerly professor of inorganic chemistry at the School of Technology at Charlottenburg, at the age of 70 years; of Dr. Wladislaw Celakowsky, professor of botany at the German University at Prague, at the age of sixty-seven years; of Dr. Latschinow, professor of physics and meteorology in the School of Forestry at St. Petersburg; of M. Dehérain, professor of vegetable physiology in the Paris Museum of Natural History; of M. Hautefeuille, mineralogist in the Faculty of Sciences at Paris, and of M. Millardet, professor of botany at Bordeaux, known for his researches on phyloxera.

THE Board of Trustees of the Carnegie Institution has made an appropriation of $8,000 for the establishment and maintenance of a desert botanical laboratory for the fiscal year 1902–1903, and the executive committee of the institution has appointed Mr. Frederick V. Coville and Dr. D. T. MacDougal an advisory board in relation to this undertaking. The proposed laboratory has been established for the purpose of making a thorough investigation of the physiological and morphological features of plants under the unusual conditions to be found in desert regions, with particular reference to the relations of the char-

acteristic vegetation to water, light, temperature and other special factors. A resident investigator to be placed in immediate charge of the laboratory will begin a series of researches upon certain more important problems outlined by the board, and facilities will be provided by the aid of which a few other investigators from any part of the world may carry on work upon any problem connected with desert plants. A discussion of the scope and purposes of the laboratory was arranged to be given before Section G at the Washington meeting of the American Association.

The Convocation of Oxford University has authorized a grant of £100 from the Craven Fund to Mr. David G. Hogarth, M.A., fellow of Magdalen, in aid of researches and exploration at Naucratis.

The Thomson Foundation Gold Medal of the Royal Geographical Society of Australasia, Queensland, will be awarded to the author of the best original paper (provided it be of sufficient merit) on each of the following subjects, the papers to be sent in by the date named: (1) The commercial development, expansion, and potentialities of Australia; or, briefly put, the commerce of Australia (July 1, 1903); (2) the pastoral industry of Australia, past, present, and probable future (July 1, 1904); (3) the geographical distribution of Australian minerals (July 1, 1905); (4) the agricultural industry of Australia (July 1, 1906).

We learn from the *Botanical Gazette* that Pearson's collections of Hepaticæ have been secured by the National Herbarium at the British Museum. It contains about 9,000 specimens, among which are many types and the material used in preparing several well-known papers.

A civil service examination will be held on January 27 for the position of assistant biologist in the Department of Agriculture, at a salary of $1,200. The subjects and weights are as follows:

Geographic distribution of animals	20
Mammals	20
Birds	15
Geography of North America	10
Taxidermy	10
Field experience in biological investigation	15
General education and training	10

On the same day there will be held an examination for the position of botanical clerk in the National Museum at a salary of $600. The scientific part of the examination is on systematic botany. On January 29 and 30 there will be held an examination to secure an eligible list of physicians in the United States and Philippine services. The scope of the examination is as follows:

Thesis (500 words to be written on one of two topics given)	4
Correction of rough-draft manuscript (250 words)	3
Mathematics (arithmetic, algebra, including quadratics, and plane geometry)	3
History and civil government of the United States	3
General history and geography	2
Colonial government and administration (general questions)	2
Political economy (general principles)	1
Education and experience	2

Optional examinations may at the same time be taken in most of the sciences. Candidates are particularly desired for positions in chemistry, engineering and agriculture in the Philippine service at salaries from $1,200 to $1,400.

Reuter's Agency is informed that the suggestion that the British Association should hold its annual meeting for 1905 in South Africa emanated from the new South African Association of Science, of which Sir D. Gill, Astronomer Royal for the Cape, is president. Before the last meeting of the British Association at Belfast invitations were sent from the municipalities of Cape Town, Kimberley, Bulawayo and other centers in South Africa, and it is understood that these have been accepted, and that the session of 1905 will be held in South Africa. Scientific papers will be read at various centers in the South African colonies, and visits will be paid to numerous places of interest. A sum of £7,000 has been collected in South Africa for the entertainment of the Association. While in Rhodesia the visitors will be the guests of

the Chartered Company, who will place their railways at their disposal, and, among other things, take them by special train to the Zambesi, where they will stay at the new hotel to be erected near Victoria Falls. Probably the guests will leave England in a special steamer.

THE second International Congress of Mathematicians will be held at Heidelberg in 1904.

THE Association for Promotion of Scientific Research by Women announces that applications should be received before March 1 for the American Women's Table at the Zoological Station at Naples. Application blanks for the use of candidates, items relating to the expense of living at Naples, and full information as to the advantages for research at the station may be obtained from the secretary, Miss Cornelia M. Clapp, Mount Holyoke College, South Hadley, Mass.

THE House of Representatives has passed the pure food bill introduced by Mr. Hepburn; it provides "for preventing the adulteration, misbranding, and imitation of foods, beverages, candies, drugs and condiments between the States and in the District of Columbia and the Territories, and for regulating inter-State traffic therein." It directs the Secretary of Agriculture to organize the chemical division of the Department of Agriculture into a Bureau of Chemistry, which shall be charged with the inspection of food and drug products, and shall from time to time analyze samples of foods and drugs offered for sale. Traffic in adulterated or misbranded goods is prohibited under penalty of a fine not exceeding $200 for the first offense, and for each subsequent offense a fine not exceeding $300 or imprisonment not exceeding one year or both.

AT a meeting of the Zoölogical Society of London on November 18 Dr. Forsyth Major read a paper on the specimens of the Okapi that had recently arrived in Brussels from the Congo Free State. The author stated that these specimens, whilst presenting the same specific characters as the specimens formerly received by the Congo State authorities, showed conclusively that the male was alone provided with horns, and that the mode of their development was the same as in the Giraffe. The Okapi seemed to be a more generalized member of the Giraffidæ than the Giraffe, sharing not a few features of alliance with the Upper Miocene *Palæotragus* (*Samotherium*). In several characters it was intermediate between the Giraffe and the fossil forms; but, apart from these, some features were pointed out in which it appeared to be even more primitive than its fossil relatives. These last characters went some way to support the assumption that Africa was the original home of the Giraffidæ.

THE London *Times* states that the official decision of Germany to take part in the Universal Exposition to be held in St. Louis in 1904 has long been assured. The delay in making the announcement has been due wholly to the exigencies of the domestic situation, and to the depression in business prevailing during the past two years. In fact, after the visit of Prince Henry to St. Louis, the tender by the Emperor of a statue of Frederick the Great to the city of Washington, and the changed attitude towards the Monroe doctrine, recently apparent, participation on large lines was certain. These have been an earnest of the emperor's desire to please and conciliate the Americans upon both the diplomatic and personal sides. High politics has, however, been only one of the influences behind this decision. The principal idea has been that of broadening the demand for German wares, with the result that there is general concurrence in the opinion both as to the necessity and the helpfulness which come from the most perfect and varied displays at all the great exhibitions. Even that at Paris in 1900 was striking, following, though it did, the failure to exhibit there at all in 1889. The great Krupp firm, which has so distinctly been built up to its present massive proportions by the policy inaugurated at the Great Exhibition of 1851 and since maintained without interruption, has really been the one potent example. At the Chicago Exhibition of 1893 Germany expended about $800,000 upon its

buildings, its official display, and as an aid to the manufacturing and commercial features. Thus far no announcement has been made of the sum likely to be set aside for use at St. Louis, but, from assurances given by the emperor, that for the purpose of illustrating every phase of its artistic, manufacturing, agricultural and industrial development Germany would make at St. Louis the finest exhibit ever shown from that country, the conclusion has been reached that at least 4,000,000 Marks will be set aside for this purpose.

Mount McKinley, the highest mountain on the North American continent, was visited last season by Alfred H. Brooks and his party from the United States Geological Survey, of which Mr. D. L. Reaburn was topographer. As far as is known, this is the first time the slopes of the peak have been reached by white men, though in 1898 its altitude and moisture were determined from a distance by Robert Muldrow, of the same survey. The mountain is located near the western margin of the Alaskan Range, the general name given to the large mountain mass which separates the Yukon and Kuskokwim waters from Cook Inlet drainage. It is a great dome-shaped mountain, formed of intrusive rock, towering to an elevation of over 20,000 feet above the sea level. Though its summit reaches so high an altitude, almost four miles above tide, it probably is not as difficult of ascent as some other Alaskan mountains, for example, Mount St. Elias, because of its relatively high snow line. As the season was well advanced, as much of his itinerary had still to be carried out, and as it was no part of the original plan, Mr. Brooks did not attempt to pass the snow line, though this point was reached. Now that the location and height of the mountain have been established by the exploration of the Geological Survey, travelers and individual explorers will doubtless soon attempt to reach the summit. In anticipation of these attempts, Mr. Brooks is preparing a description of the country, giving routes by which the mountain may be reached and other information valuable to those interested in its ascent. His paper will appear in one of the leading geographical magazines. The more elaborate and extended report of the exploration will be published by the Geological Survey at an early date.

UNIVERSITY AND EDUCATIONAL NEWS.

It is announced that during the past two years an endowment fund of more than $1,-000,000 has been raised for Syracuse University.

By the will of the late Mrs. Lura Courrier, of New York City, Yale University will ultimately receive $50,000 for the aid of poor students.

Northwestern University will celebrate its founder's day on January 28, when its new professional school building, costing over $900,000, will be dedicated. President Hadley, of Yale University, will deliver the dedicatory address, taking as his subject, 'The Place of the Professionl School in the Modern University, and its Relation to the Other Departments.'

The electrical laboratory of the Rensselaer Polytechnic Institute at Troy, N. Y., has been almost completely destroyed by fire. The loss is estimated at over $30,000.

We learn from *Nature* that the reader in geography and the lecturers in ethnology and geology of Cambridge University have arranged for a series of lectures and practical courses to serve as a training for persons wishing to undertake exploration or desirous of contributing to our knowledge of foreign countries. The series will be held during the Lent term, and will include history of geographical discovery, principles of physical geography, map-making and map-reading, geography of Europe, by Mr. Oldham; anthropogeography, practical ethnology, by Professor Haddon; geomorphology and geology, by Mr. Marr; plane-table and photographic surveying, by Mr. Garwood, and elementary astronomical surveying, by Mr. Hinks.

Kenyon K. Butterfield, instructor in rural sociology at the University of Michigan, has been appointed to the presidency of the Rhode Island State College of Agriculture, at Kingston.